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THE FLOW OF WATER OVER SUBMERGED WEIRS.

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I.

Under the title "The Flow Through Submerged Outlets," there appeared in the June number of this Magazine, a translation of an article by M. Alfred Salles, published in the "Memoires of the Bureau des Ponts et Chausées" for 1884, —which will grievously mislead those who take it as embodying the present state of knowledge on this subject.

The writer attributes to M. Mary, under date of 1860, the following formula, for the delivery through partly submerged sluices,

$$Q = mLH'\sqrt{2g \times (H - H' + h)}$$

in which L is the length of the sluice, H is the height of the water on the up-stream side above the sill of the sluice, H′ is the height of the surface on the down-stream side above the same level, h is the height due to the velocity of the current on the up-stream side, and m is a coefficient not experimentally determined, but assumed to be about 0.8.

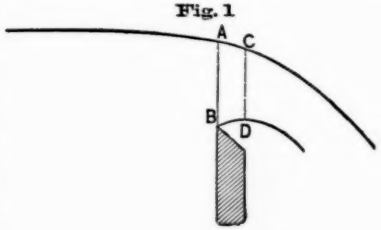
M. Salles then substitutes in this formula, numerical data obtained at a dam across the river Garonne, at Toulouse, and deduces 0.82 as an approximate value for m, "agreeing remarkably with M. Mary's anticipation."

A brief examination will show that this agreement is purely accidental.

This formula makes the value of Q depend directly upon that of H′, or the height of the backwater,—it takes H′ as the depth of the stream. It is obvious that if the channel below the dam were deepened so as to lower the backwater, other data remaining the same, Q would be increased; but this formula makes it decrease. Drawing the backwater down to the crest of the weir, in this way Q would increase while the formula would decrease it to nothing.

This absurdity results from confounding the case of flow through a *sluice* with the radically different one of flow over a weir. For the latter case, the formula in question misrepresents both factors of the discharge—the depth of stream, as above shown—and also its velocity. In the case of free discharge over a weir, it is universally admitted, and indeed is easily proved, that the velocity of discharge, at AB or CD (Fig. 1), increases

Fig. 1



from the surface downwards. In flow over a submerged weir, the velocity must

also increase, similarly, down to the *backwater* surface; but there is no reason to believe that it increases down through the remainder of the depth to the crest of the weir, where the discharge is made against still or slow-moving water. Hence it is all wrong to assume, by using this formula, that the mean velocity for the whole depth flowing over a submerged weir can be represented by

$$\sqrt{2g(H-H'+h)}$$

(or any constant fraction thereof), as a general expression, where H′÷H can have any value less than unity.

This unfortunate confusion appears to have originated with Lesbros, before 1850, and in consequence of it, his writings and those of his followers have obscured the subject, instead of elucidating it, as a brief résumé of the progress of knowledge in regard to it will show.

We will continue to use the same symbols as heretofore, but will eliminate L, by taking q=the discharge for a unit of length of weir=Q÷L; and will substitute F for H−H′, representing both the height of the free section and the fall, where that is most convenient.

Dubuat had a good understanding of this subject more than 100 years ago. In his "Principes d'Hydraulique," t. I., §§ 141–147, editions of 1786 and 1816, he elaborates the general theory, dividing the stream into a free section acting like an unobstructed weir with velocity increasing from the surface downwards, and a submerged section acting like a rectangular orifice of the height H′ and under the head

$$(F+h).$$

He also describes the dam or weir with which he made a single experiment for obtaining a coefficient of discharge to be used in his formula.

D'Aubuisson, in his undated treatise on "Hydraulics," published probably before Lesbros' "Memoire," quotes Dubuat, to this effect, adopts this division of the stream, and proposes coefficients deduced from his own experiments. His corrections for velocity of approach are not well managed, however, and we may simplify the matter by eliminating them for the present; *i.e.*, taking the case of discharge from still water. His formula hen becomes, for English feet,

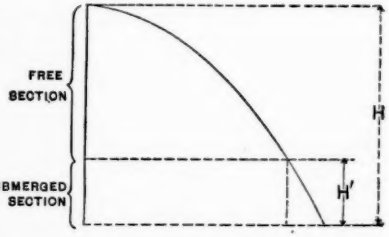
$$q = 3.48\ F^{1.5} + 4.98\ H'\sqrt{F}.$$

According to the best modern experiments, as will appear further on, an expression of this form will represent the discharge very closely, *i. e.*, the coefficients for it are nearly constant, but their mean values are about 3.33 and 4.59 respectively, for a weir having a vertical sharp-edged face of sufficient height to produce complete contraction. Hence D'Aubuisson's formula gives results which may be from 4½ to 9 per cent. too large for standard weirs, but are not far wrong for many practical cases where the contraction is somewhat less.

Other eminent experts, before 1850, adopted this general theory, with some variations in the coefficients and in form of statement. Among these were Eytelwein, in his "Mechanik fester Körper und Hydraulik," published in 1800, 1822 and 1842; and Weisbach, in his "Mechanics," published in 1846.

These two writers assume the coefficient of discharge of the free section at two-thirds that of the submerged section, as indicated by the parabola of velocities (Fig. 2), which reduces the formula to

Fig. 2



$$q = n\left(\tfrac{2}{3}F^{1.5} + H'\sqrt{F}\right)$$

or

$$q = c\sqrt{F}\,(H+0.5H')$$

Lesbros' experiments were made in 1828–1834, mainly with reference to free discharge through orifices, and the section of his report relative to "Dépenses des Déversoirs Incomplets ou en Partie Noyés" appears to be a sort of by-product.

He brought the water to some of his orifices through a long, rectangular open channel, supplied through a square-edged aperture, of the same width and depth, in the vertical side of a reservoir, with

its bottom in the same level plane with the bottom of the channel and aperture (Fig. 3), and his discussion is based upon observations of the conditions of flow through this aperture.

In beginning his examination of the subject, Lesbros refers to Dubuat's "Principes, t. I. §§. 141–147, but instead of taking the theory there set forth, he turns to t. II. §. 413, etc., where Dubuat describes the small weir (0.11 m. high and 0.47 m. long), with which he made four experiments with free discharge, and one with the flow obstructed by *backwater*. For the case of free discharge Dubuat concludes that q varies with $(H+h)^{1.5}$. From this, Lesbros appears to have evolved the formula for submerged weirs,

$$q=m(H+h)\sqrt{2g(F+h)},$$

which he says—falsely so far as I see—that Dubuat *indicates*. Having thus ignored Dubuat's whole theory as stated in §§. 141–147, and set up in his name a a formula utterly inconsistent with it, he undertakes to demolish the latter by saying that if it expresses the true law of the phenomena, it is evidently suitable for *déversoirs* formed at the entrances of canals like those with which he experimented. *i. e.*, with apertures starting from a level floor, because the escaping stream is partly submerged in both cases, whereas he finds, by putting the data from his aperture experiments into it, that the resulting values for the coefficient vary *wildly*, from 4. to 0.287.

It seems that he reaches this result by a process which Dubuat probably would have condemned, that is, by taking H′ at the highest place in the channel below the aperture, sometimes at the lower end, see Fig. 3; for he announces as a result of his study of the subject, that instead of this, it ought to be taken at the lowest place, in the depression at the foot of the fall. Taking it in this way, and neglecting h, which was unimportant in his experiments, he represents the discharge by the expression $H\sqrt{F}$, with a coefficient varying with the relative values of H and H′, but entirely independent of their absolute values, or the *scale* of the experiment.

But his apparatus was radically different from a *weir*, and manifestly unfit for showing the laws of flow over one.

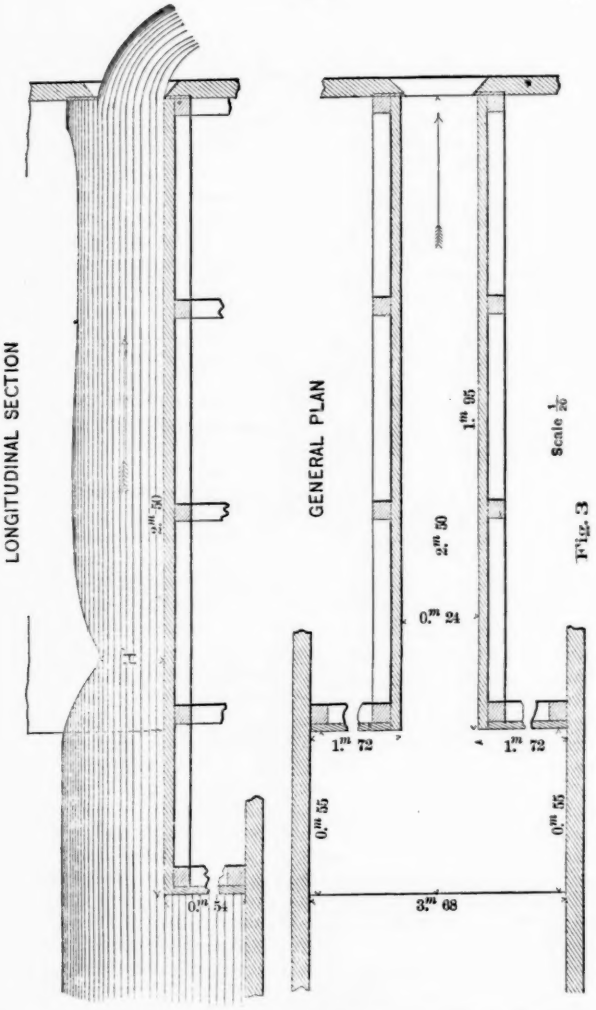
One characteristic of a weir is that it produces bottom contraction; and contractions are suppressed, or else the narrowing of the stream caused by them is rebated directly from the length of the weir. With this apparatus there was no bottom contraction, but instead thereof, two end contractions, with falls varying from .0027 m. to 135 m.

In fact, without these end contractions there would have been no sudden fall here, only the slopes appropriate to flows in a uniform channel.

Neither was there any deepening of the channel down-stream from the aperture; hence the water, instead of falling into a deep channel of escape, had to flow over the level floor. Thus, the depth down-stream from the aperture affected q, not merely by limiting the fall, but also as being the depth of a section through which all the water had to pass after leaving the aperture. H′ cannot be reduced to 0, because no water can flow in a stream of no depth, though Lesbros exterpolates coefficients out to this impossible extreme.

Probably Lesbros, in trying to simplify the expression, reasoned that all the water finally passes under the accelerating action of the net head F (in reaching the section of minimum depth H′), hence that this fall should be taken as acting on the whole stream, but apparently he was aware that the resistance here was less than that against flow into still water, though he left the effect of this and other neglected factors to be covered by the variations in the coefficient. In fact the discharge was made against water moving in the same direction towards an orifice opening into the air. With the water in the reservoir maintained at any given height H, the two components into which it is divided, fall of surface and depth at H′, are affected by the more or less resistance to the escape through the channel and orifice. As this resistance diminishes, the depth in the channel diminishes and the velocity there increases. The resulting diminution of resistance to flow at H′ allows the velocity there to increase faster than $\sqrt{F+h}$. But the actual velocity of the stream at this place is difficult of determination, in consequence of the varying narrowing of the effective area by the end contractions,

Fig. 3



and the varying curvature of the surface crosswise of the stream.

If this curvature makes the mean depth less than H′, the mean fall is greatter than H—H′, but Lesbros ignores this effect.

Thus the mean depth will sometimes vary from H′, the effective width will vary, sometimes widely, from the full width of the channel, and the velocity will not bear a constant relation to

$$\sqrt{F+h}.$$

For these reasons, Mary's formula is unsuitable for this case, though it cannot be run into the absurdity of supposing discharge without depth. If we attempt to apply it to Lesbros's data by assuming streams of the full width of the channel and of the depth H′, we make merely mathematical transformations, taking cross-sections reduced in the ratios of H′ to H and coefficients increased in the same ratios, respectively. But it is interesting to note the results of such transformations, as exhibited below:

	From Lesbros' Experiments.					Exterpolated by Lesbros.			
Values of H′÷H..... ...	.95	.85	.75	.65	.56	.50	.30	.10	0
L.'s coefficient.....	.522	.512	.502	.492	.481	.474	.444	.409	.390
M.'s "	.55	.60	.67	.76	.86	.95	1.48	4.09	Infinity.

Lesbros' exterpolations evidently run wild.

The actual velocity at the smallest section was apparently materially above 86 per cent. of that due to the fall, where that coefficient appears, as the cross section was probably materially reduced by end contractions.

This formula is also worse than Lesbros' in the case of a submerged weir, where H′ can vary independently of H down to 0 or minus, and where it involves the absurdity of diminishing q to 0, as the backwater resistance disappears.

Mary's supposed teachings should not be criticised in the absence of his own statement. The hypothesis most creditable to him is that he did not intend his formula for submerged weirs, but for some form of sluice different from Lesbros' apparatus.

M. Salles computes the flow in the Garonne with his data, and Lesbros' formula and coefficient, and finds the result far too small. If he had corrected for velocity of approach, as Lesbros doubtless intended, the discrepancy would have been much less, though still large.

But the dam was nowhere near high enough to produce complete contraction. According to the data given, the depth up stream from the dam could not have exceeded $6\frac{3}{4}$ meters, while the surface was about 6 m. above the crest; hence the dam was only about $\frac{3}{4}$ m. high. Whatever its form of cross-section, it would be but a slight obstruction to a stream nearly 9 times as deep. Hence, the discharge was much greater than it would be over a normal weir producing full contraction. If this dam produced a surface fall of about 1 m., as stated, we must infer that its height on the down-stream side was much more than $\frac{3}{4}$ m., that the channel was much lower there than on the up-stream side.

It is evident that French writers of the last half century have made an unfortunate departure backwards in neglecting the teachings of Dubuat and D'Aubuisson and following the line here exposed.

II.

In Germany, painstaking but unsuccessful investigations of the subject were made by Bornemann, between 1866 and 1872. He attempts to represent his results by various complicated formulas, but the coefficients which he deduces differ so widely as to show conclusively that one or more of the principal factors is not properly represented in these formulas.

But it is highly improbable that his facts are all wrong. There is a strong presumption that his statements of heights and quantities discharged are mainly correct. Let us examine them with the aid of graphic representation:

For this purpose, I plot the values of H as ordinates, and those of H′ as abscissas, in a rectangular system, the common origin representing the crest of the weir. At each point whose co-ordinates represent the heights observed in an experiment, conceive a perpendicular erected from the plain of the paper, and of a height representing the corresponding value of q. Erecting a vertical in this way for each experiment, the upper ends will determine points in a curved surface representing the values of q for all values of H and H′ within the *field* or range of the data. Values of q for intermediate points may be interpolated, and curved lines may be drawn in the surface for constant values of q, with any degree of minuteness which the exactness of the data and the scale of the drawing will justify.

Fig. 4 gives part of Bornemann's results translated into English feet, and shown in this way, by broken lines, with the heights of weirs W, and the values of q written in them. It also shows the true co-ordinates for four of these values of q with normal weirs, by full lines, deduced from American experiments of the highest authority.

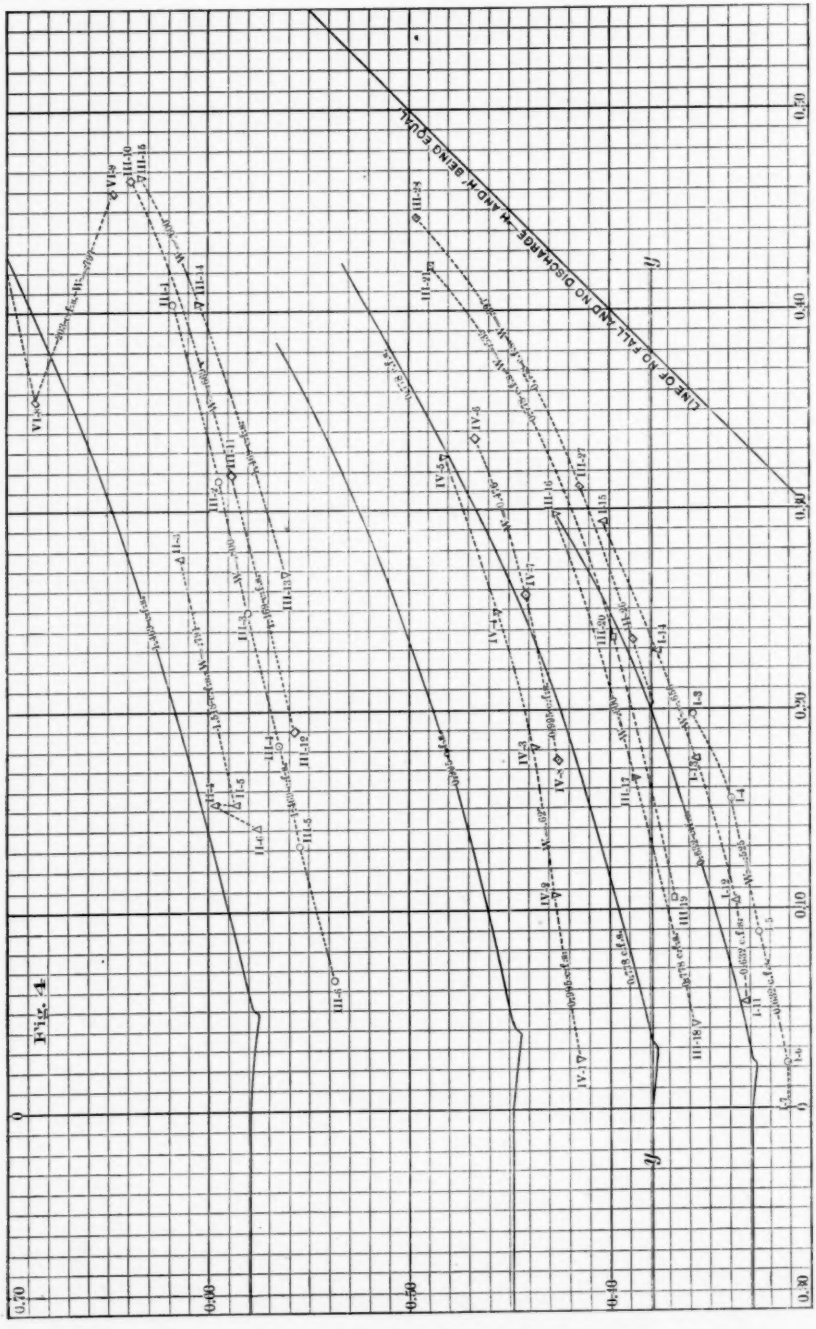

LINE OF NO FALL AND NO DISCHARGE — H AND H' BEING EQUAL
0,70
0,60
0,50
0,40
0,30
0,20
0,10
0,00
0,90
0,80
0,70
0,60
0,50
0,40
0,30
0,20
0,10
0,00

Fig. 4.

To the left of the origin the values of H′ are minus, or downward from the crest of the weir, and cease to affect the discharge materially. In this part of the field, then, the lines of constant discharge become straight.

In looking at this diagram, we are forcibly reminded of the obvious but important fact that a formula may disagree with some of the results of experiment without being wrong—that the latter are inexact. It is evident that vi. 9, and two of the group ii. 4, ii. 5 and ii. 6 are materially *out*, perhaps from clerical or typographical errors. Several other points also appear to be slightly out, judging by their incongruity with their neighbors, which is a better test than comparing them with distant points by the aid of a theory which is itself the object of the investigation.

It will be seen that the discharges for given values of H and H′ were all considerably larger than those for a normal weir, as shown by the full lines.

It is obvious at a glance that the height of the weir W was an important factor, and that the variations of q for identical values of H and H′ were caused by the variations of W; also that the excesses over the discharges for a normal weir were probably due to the insufficient heights of the weirs.

An essential characteristic of a weir is that it causes the stream to contract vertically, requiring increase of velocity and fall to produce this increase. It is obvious that a barrier just beginning to rise across the bottom of a channel will just begin to deflect and contract the stream; as it rises higher this effect will increase, but not indefinitely. For Bornemann's values of H, it is probable that the full effect of contraction, or very nearly the whole, is reached when $W=2\frac{1}{2}H$. Up to the point of complete contraction, raising the weir increases H, or else diminishes the discharge, by increasing the contraction, also by increasing the depth and thus diminishing the velocity of approach.

Doubtless the discharge of a submerged section is also increased by velocity of departure, as suggested by Fteley and Stearns, where the stream is not much deflected after passing the crest; hence discharges against water having a considerable horizontal component of motion, instead of against that which has little or no motion down stream.

After full contraction is developed, further raising of the weir increases H only by changing these velocities, and to a small extent.

When $W=2\frac{1}{2}H$ the crest is above the backwater and we have free discharge into air, but submergence can, of course, be produced, and the value of H′ can be varied at will, by obstructing the flow on the down-stream side.

The simplest way to test Bornemann's experiments, in this regard, seems to be as follows:

Take a line of experiments with q constant, say .778*cfs*, and take H constant at .3793=H for this q with normal weir, free discharge and no velocity of approach ($V_a=0$). Make a diagram (Fig. 5) in which the abscissas represent heights of weir W, or, rather, the deptns of the bottom of the channel below a permanent crest, beginning with O and increasing to the left. Let the ordinate at the origin represent the constant value of H=.3793, and the ordinates to the left of the origin represent the decreasing values of H′ necessitated by the increasing values of W, ascertained by drawing the horizontal line *yy*, Fig. 4, at the height H=.3793, and noting the values of H′ at which this line intersects the curves of constant discharge (q=.778). Plotting these values as ordinates at the corresponding values of W, we find a line indicating the diminutions in the values of H′, and hence the increments of fall required to balance the increasing contraction from increase of W and the slight falling off in velocity due to increase of depth with increase of W.

Assuming that the full effect of contraction would have been developed at some stage between W=.80 and W=1.10, the corresponding depths would have varied from 1.18 to 1.48, and the values of V_a from .66 to 0.53; and I estimate that the velocities of approach for this part of the diagram would have been sufficent to balance the effect of backwater up to the curve *zz*. Wherever complete contraction occurs, the line limiting the heights of H′ will descend to and unite with the line *zz*. The data is insufficient for fixing this point exactly, but apparently the lines cannot be far apart to the left of $W=.95=2\frac{1}{2}\times.3793$.

and perhaps they meet a little to the right of this.

The ordinates to the curve show approximately the values of H′, or heights of backwater) which balance the effects of velocity of approach and defect of contraction, as the depth below the crest varies from 0 to 0.95±. Or we may regard the complements of these ordinates (.3793−H′) as the falls which are needful, in addition to the velocities of approach, to produce the discharge.

Velocity of approach, V_a, is an inseparable element in the operation, varying inversely with the depth. Where W=0, there is no contraction, hence no fall is required to produce acceleration therefor, H′=H, and V_a accounts for the whole effect. As W increases, V_a diminishes and F increases, tending toward the limit, V_a =0, F the sole *motive*.

Some idea of the relative importance of the velocity component may be gained from the following considerations. At the origin,

where

W=0, depth=.3793, V_a =2.05, h=.065;

where

W=.95, depth=1.33, V_a =.585, h=.0053.

We find from Fig. 4 that when W=.60, H′=.135. From the American experiments we know that, for a normal weir without velocity of approach and H and H′ as here, q=.71 instead of .778, difference, .068. The velocity of approach probably accounts for nearly half of this, leaving the other half, more or less, for the increase by defect of contraction at this place.

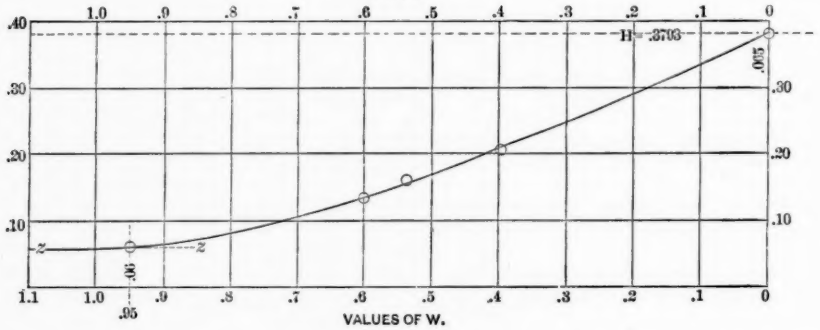
Similar investigations of Bornemann's data with other values of q and H, give similar results, but with some irregularities.

The conclusion seems irresistible that Bornemann's weirs were too low, and that their varying deficiencies in this respect will account for the variations not otherwise accounted for or reasonably chargeable to error.

This introduction of one unnecessary variable, W, throws his experiments into the class of intermediate cases—they have little or no value for the general case—for ascertaining the laws of flow over a normal weir.

It would now be possible to collate Bornemann's results together and apply corrections fixing the most probable values. Such changes as would thus be introduced into the data would, of course, affect the foregoing investigation and diagrams, but would not materially change the general results. I take the figures as they stand, and give the results as approximations, not as the closest that could be deduced from the data with unlimited labor.

Fig. 5



III.

American Experiments.

Data incomparably better and fuller than any other known to the writer have been furnished by

a. Six experiments made by James B. Francis in 1848.—"Lowell Hydraulic Experiments," p. 99.

b. Twenty-two experiments made by Messrs. Fteley & Stearns in 1877.—

"Proceedings of the American Society of Civil Engineers" for 1883, and

c. About fifty experiments by Francis in 1883.—Proceedings of same society for 1884.

Fteley & Stearns, of course, did not have the benefit of the latter series of experiments, in their discussion. Taking the formula

$$q = c\sqrt{F}\,(H + 0.5\,H') \qquad (1)$$

or $$q = c\sqrt{F}\,(F + 1.5\,H') \qquad (1')$$

they find values of c varying from 3.372 to 3.089, for varying values of $H' \div H$. The later experiments indicate that it also varies slightly with the absolute value of H—the maximum variation in whole range of the experiments being about $2\frac{1}{2}$ per cent.

Francis finds that the coefficients become nearly constant at the values 3.33 and 1.381, so that the formula

$$q = 3.33\,\sqrt{F}\,(F + 1.381\,H') \qquad (2)$$

or $$q = 3.33\,F^{1.5} + 4.5988\,\sqrt{F}\,H' \qquad (2')$$

follows the data with a good approach to accuracy; the discrepancies being partly due to small errors in the data, but not wholly.

It is known that at certain stages of backwater, in the vicinity of $H' = 0.08\,H$, its presence increases the discharge slightly, instead of decreasing it, and the formulas (2) (2') do not take account of this effect. It appears also that as $H' \div H$ increases, the second coefficient slowly diminishes to 1.35 ± and then slowly increases again; though the values of q found by taking the medium value 1.381, differ but slightly from the truth; not over 1 or 2 per cent. at the maximum.

In these experiments, the minimum value of $W \div H$ (with free discharge) appears to have been 2.71. The values of H varied from about 4 in. to about 2 ft. 4 in., and the values of H' from O up to $\frac{1}{2}$ H and in some cases to .7 H or more; and the field is well covered for practical purposes, up to and considerably above these limits.

But a further study of it may have some scientific interest, and will, at least, show where more experiments are desirable for perfecting the theory.

In all cases, the water was delivered to the channels in which the weirs were established, through fixed orifices, under large heads, so that small accidental variations of fall produced but slight changes in the efflux. These small variations of fall were closely noted and their effects on the discharges were allowed for in the usual way. The discharge for some particular fall was computed from values of H when the backwater was below its range of effect. Then keeping the discharge as nearly constant as practicable, the backwater was raised step by step, and the corresponding values of H' and H were noted.

The greatest mean velocity of approach appears to have been about 1.31 ft. per second.

Keeping the dischange constant and varying H' and H, as above, the effect of the velocity of approach remains very nearly constant, and hence no correction was made for it, as I understand. If the velocity of approach had been made indefinitely small by deepening the channel, the quantity, with free discharge and any observed value of H, would have been as reckoned; and it is assumed that with this actual discharge maintained without velocity of approach, the increasing series of values of H' and H would also have been found identical with those actually observed. I estimate that this cannot make an error of one-tenth of one per cent., hence is unimportant.

By plotting the data, with co-ordinates of the full size, in the same way as described for Bornemann's experiments, we bring the results within easy grasp and greatly facilitate the examination and weighting of them.

It is needful, however, to eliminate the small accidental variations in the quantity discharged. For this purpose, we may observe that all lines with positive co-ordinates and making angles of 45° with the axes, are lines of constant fall. According to formula (2') all vertical planes erected on such diagonals intersect the curved surface which represents q, in straight lines having rates of rise equal to $4.5988 \times \sqrt{F}$. In fact, these intersections are not exactly straight lines, the coefficient not being quite constant; but for these small adjustments, the constant 4.6 is near enough.

Taking, for instance, the series of ex-

periments where q was taken at the average value of 7.202, we find in Francis's experiment No. 43, $q=7.174$ or 0.028 less. $F = 1.254$. $.028 \div 4.6 \sqrt{1.254} = .0054$. Hence q would be increased from 7.174 to 7.202 by increasing H and H′ .0054 each and the co-ordinates so found will fix a point in the curve of $q=7.202$.

Where H′ is minus, the adjustment is made on the value of H only.

Having thus adjusted all the points at which q differed materially from the average of its series and plotted the results with all the others, it is useful to write at each point the values for the variable coefficients deduced from the experiment there represented.

On comparing either formula with the data as thus exhibited, it is apparent that there are some small outstanding variations of coefficients which the formulas do not take into account, as stated in the first part of this section. They are complicated, of course, with the small inaccuracies of the data. The latter may be eliminated approximately by the laborius empirical process of drawing an estimated mean line through each series of points and adjusting to regularity. With the large quantities corresponding to the upper part of the diagram, we may expect inaccuracies of .01 or .02 ft. in the values of H and H′, and correct curves of formulas may vary from some of the points to that extent. The surface fixed by these curves should be *regular*, or free from irrational crooks, in all directions. We may test it by means of series differences or otherwise, and the crooks so found should be eliminated by altering the curves, with due regard to the equality and distribution of the plus and minus variations from the data. Series for this purpose may be taken on lines running across the field in any direction; but lines with judiciously selected values of variables are better than others.

In this way we test and determine the correction for each point mainly by its congruity with the points in its vicinity, their influence diminishing as their distance increases, instead of attempting to give distant points equal weights by the aid of any theory as to the relations of q to H and H′.

A study of a diagram so worked up develops several interesting relations and hypotheses, more or less clearly; but further experiments of extreme precision are needed for perfecting the whole theory.

For practical purposes, however, a well-adjusted diagram of this kind, will be substantially cleared from probable errors of data and from the corrections for the small variations of the coefficients.

With a line of constant discharge for each 0.10 *cfs* in the upper part of the diagram and for each 0.05 *cfs* in the lower part, it is easy to interpolate by estimation to one-tenth of these quantities respectively, hence, to read off the value of q for any values of H and H′ within the limits of the diagram, with substantially all the precision which our experimental data will justify, and more than will ordinarily be attained in practice in the ascertainment of H and H′ and in allowing for velocity of approach. That is, the results will be more exact than would ordinarily be obtained in any other way and about as good as it is possible to obtain with existing data.

CARRYING COALS TO NEWCASTLE.—Carrying coals to Newcastle has hitherto been held to be an absurd proceeding; but it will no longer be treated in that contemptuous fashion if what we hear from Staffordshire is correct. It is stated that a new feature of competition from Spain with English iron-masters is just now making its appearance in Staffordshire. It is the introduction into that market of Spanish hematite pig iron. This is pretty much the first time that Spanish hematites have been offered in this country, the Spaniards having been previously content to supply English iron-masters with their rich iron ores from the Bilbao and other mines, wherewith to make hematites in this country, and heavy shipments of these ores are imported weekly into South Wales, the North of England, and some other of the English centers. The Spanish hematite iron which is just now being offered, is imported from Somorrostro, and a richness is claimed for it which, it is alleged, is in advance of that possessed by the ordinary English hematite pig irons. The analysis of the new pig is stated to be as follows: Silicon, 0.2; manganese, 0.85; sulphur, 0.029; phosphorus, 0.033; pure iron, 97.083. The agents who are introducing the foreign iron are, we are informed, authorized to sell at a low price, and they will make an effort to get a footing upon the English iron markets. An evident moral is to be drawn from the above. If pig iron can be imported at a profit into England from abroad, why not, it may be asked, some day coal?

THE PROPOSED ABOLITION OF THE ASTRONOMICAL DAY AND ITS BEARING ON PROBLEMS OF NAVIGATION.

By Professor STIMSON J. BROWN, U.S.N.

Proceedings of the United States Naval Institute of Annapolis, Md.

With the growth of commerce and the vast extension of telegraph and railroad systems, the necessity of a reform in the usual methods of time-reckoning has made itself forcibly felt. This has expressed itself in various attemps to adopt systems of standard time which should embrace as wide an extent of longitude as possible, and at the same time vary but little from the local time of any place using one of these systems. Thus, it was a comparatively easy matter for Great Britain, in 1848, and Sweden, in 1879, to adopt as legal standard times, the mean solar day beginning at midnight of the meridian of their respective Government observatories. The solution of the problem in the United States and Canada has been more difficult, on account of their wide extent in longitude. Yet, at a railway time convention in 1883, a system was adopted, and shortly after carried into execution, which system embodies all the essential principles necessary for uniformity in the chronology and time-reckoning of the ordinary affairs of life. It will be remembered that this plan divided the two countries into districts by meridians one hour of time apart; that the division into districts was so made that the central meridian of each is an integral number of hours west of the Greenwich meridian; and that from the central meridian of each district the standard time of the whole district is to be reckoned.

In Continental Europe, with railroad, telegraph and postal routes extending through different countries separated by purely artificial boundaries, the inconvenience has been even greater than in the United States; but national pride and jealousy have proved serious obstacles to systematic reform. Chief among these has been the necessity, in any thorough reform, of the selection of an initial meridian which would be universally recognized.

Out of the discussion which these attempts have involved, has developed the idea of a universal day. Such a day would begin, for the whole world, at a given instant of time at the initial meridian; but by this scheme the various epochs of the day would begin one hour later for each hour of longitude to the west, and there has been no serious attempt to adapt it to the practical affairs of everyday life. The railway-time system of the United States is only a simple modification of this idea. With standards of time carried out for the whole world on this system, the process of finding the universal date and time would be a very simple one, and the variation of local date and time reduced to a simple rule.

The necessity of a universal prime meridian, met in all the proposed plans for securing uniformity in time-reckoning, has always been urged by sea-faring men for a different purpose. It is not necessary to more than refer to the utility and convenience to navigators of the adoption of a universal zero point from which to reckon longitude. The need of this has been a prominent factor in the success of the movement toward uniformity in the methods of time reckoning.

The various commercial and scientific societies of Europe, in which these questions have been discussed, have from time to time suggested the United States as the most favorable nation, for obvious reasons, for taking the initiatory steps in calling up an international conference for selecting this prime meridian. The subject, in this country, had been so generally discussed, and the utility of the proposed reforms so generally recognized, that Congress, in 1882, passed an Act authorizing the President to call an international conference for the purpose of carrying the project into execution. After the arrangement of the necessary diplomatic preliminaries, the Conference met in Washington in October, 1884. All its

acts are of special interest to nautical men, one of them proposing no less radical a reform than the abolition of the astronomical day.

The second resolution reads as follows: "That the Conference proposes to the Governments here represented the adoption of the meridian passing through the center of the transit instrument at the Greenwich Observatory, as the initial meridian for longitude." The vote of San Domingo was the only negative vote; the delegates from France and Brazil abstained from voting.

The third resolution was, "That from this meridian, longitude shall be counted in two directions up to 180 degrees, east longitude being plus and west longitude minus." This reversal of the uniform custom of navigators in regard to the sign for the application of longitude arose from its application in reducing universal time to local time; it is of little importance to navigators, as the precepts for its application to local or Greenwich time are too simple to cause any confusion as to the meaning of the resolution.

The third and fourth resolutions were in regard to the universal day, defining it as beginning at the moment of mean midnight of the initial meridian, coinciding with the beginning of the civil day and date of that meridian, the hours to be counted successively from 0 to 24.

In the sixth resolution, "The Conference expresses the hope that as soon as may be practicable the astronomical and nautical day will be arranged everywhere to begin at mean midnight."

This resolution, though anticipated, has met with strong opposition from the majority of eminent astronomers, particularly Europe. It is not difficult to see why they should oppose any innovation of the long-established and natural method of counting astronomical time. No one has so forcibly presented the objections from an astronomical point of view as Professor Newcomb; I therefore quote at length his letter to Commodore Franklin, protesting against the adoption of the new system, January 1, 1885:

"The first of these recommendations proposes a change in the method of counting astronomical time which has come down to us from antiquity. The practice of taking noon as the moment from which to count the hours originated with Ptolemy. This practice is not, as some distinguished members of the Conference seem to have supposed, based solely upon the inconvenience to the astronomer of changing his day at midnight, but was adopted because it was the most natural method of measuring solar time. At any one place solar time is measured by the motion of the sun and is expressed by the sun's hour angle. By uniform custom, hour angles are reckoned from the meridian of the place, and thus by a natural process the solar day is counted from the moment the sun passes over the meridian of the place, or over the standard meridian. For the same reason the sidereal day is counted from the moment the vernal equinox passes over the meridian of the place, and thus the two times correspond to the relation between the sun and the equinox.

"It would appear that the Conference adopted the recommendation under the impression that the change would involve nothing more than the current method of reckoning time among astronomers, and could therefore be adopted without serious inconvenience. . . .

"A change in the system of reckoning astronomical time is not merely a change of habit, such as a new method of counting time in civil life would be, but a change in the whole literature and teaching of the subject. The existing system permeates all the ephemerides and observations which fill the library of the astronomer. All his text-books, his teachings, his tables, his formulæ, and his habits of calculation are based on this system. To change this system will involve a change in many of the precepts and methods laid down in his text-books.

"But this would only be the beginning of the confusion. Astronomical observations and ephemerides are made and printed not only for the present time, but for future generations and centuries. If the system is changed as proposed, the astronomers of future generations who refer to these publications must bear this change in mind, in order not to misinterpret the data before them. The case will be yet worse if the change is not made by all the ephemerides and astronomers at the same time epoch. It will then be necessary for the astronomers o

the twentieth century, using observations and ephemerides of the present, to know, remember, and have constantly in mind a certain date different in each case at which the change was adopted. . . .

"It is difficult to present to others than astronomers who have used the published observations, the confusion, embarrassments and mistakes that will arise to their successors from the change. The case can be illustrated by saying that it is of the same kind—though in less degree than—the confusion that would arise to readers and historians in the future, if we should reverse or alter the meaning of a number of words in our language, with a result that the reader would not know what the words meant unless he noticed at what date the book was printed. . . .

"The change will affect the navigator as well as the astronomer. Whether the navigator should commence his day at noon or midnight, it is certain that he must determine his latitude from the sun at noon. The present system of counting the day from noon enables him to do this in a simple manner, since he changes his own noon into the astronomical period by the simple addition or subtraction of his longitude. To introduce any change whatever in the habits of computation of uneducated men is a slow and difficult matter, and is the more difficult when a complex system is to be substituted for a simple one. I am decidedly of the opinion that any attempt to change the form of printing astronomical ephemerides for the use of our navigators would meet with objections so strong that they could not be practically overcome."

The objections from an authority of such eminence in astronomical matters seems to be shared by the majority of astronomers in Europe; but it should be noticed that these objections are raised only in so far as they affect astronomers and their work. At the last meeting of the Astronomische Gesellschaft (at Geneva, in August, 1885), the discussion of the 6th resolution of the Conference was limited by the emphatic declaration of the President to its consideration from a purely astronomical standpoint; thus limited, although no formal resolution of the society was adopted, a large majority expressed themselves as opposed to the change. Those speaking in favor of the change were, however, among the most eminent astronomers of the society; they recognized the difficulties it would entail to astronomers, but were willing to make the sacrifice for the sake of gaining uniformity in methods of reckoning time; to these may be added Professor J. C. Adams, of Cambridge; Professor Christie, Astronomer Royal of Great Britain, as well as Professor Oppolzer, of Germany. The last named proposes to give practical effect to his views by adopting the new reckoning in an extensive list of solar and lunar eclipses which he is now preparing for publication. Professor Adams pointed out in the proceedings of the Conference that there were noted exceptions even to the universality of the old method; such, for instance as Delambre's Tables of the Sun, Burg's, Burkhart's, and Damoiseau's Tables of the Moon; Bouvard's Tables of Jupiter, Saturn and Uranus; in all of which mean midnight is used as the epoch of the tables. Also La Place, in his "Mecanique Celeste," uses Paris mean midnight as the origin of the astronomical day.

American astronomers have been as unanimous in favor of the change as those of Europe against it. Shortly after the Conference, Commodore Franklin, Superintendent of the Naval Obervatory, sent out a circular letter soliciting expressions of opinion on the subject. So far as I have been able to learn, with the exception of Professor Newcomb's letter above quoted, favorable answers were returned. Most of them, however, and this may be said of all who favor the change, advise waiting until a certain date can be fixed upon by international agreement. Nautical almanacs are published, or in course of publication, up to 1890, and the change could not well be carried into effect before that date. Professor Tietjen, who directs the publication of the *Berliner Jahrbuch*, has said that, in his opinion, such a change would not find place there before 1900. Thus it will be seen that astronomers are about evenly divided as to numbers if not as to ability.

It may be mentioned here that the change was formally adopted at the Greenwich Observatory, January 1, 1885, thus initiating the confusion liable to

arise from the selection of different dates for the inauguration of the change, the dangers of which are so forcibly pointed out in Professor Newcomb's letter. At the same time the adoption of this action by so prominent an observatory will tend to compel its ultimate adoption by all.

An examination of the opinions of astronomers will lead, I think, to the conclusion that those astronomers who are chiefly engaged in combining and discussing the vast mass of observations from various sources and widely different times, are generally opposed to any change; while those who perform the practical work of making the observations are as unanimous in its favor. To the latter the change would be a simple one, and give rise to little or no inconvenience. To the former the inconveniences introduced and the liability to error would be felt for a long time. It would seem, though, that they to whom the change might prove burdensome, are fitted by their skill, education and training to avoid the mistakes to which there would be liability.

However, it is not the purpose of this article to discuss the opinions of astronomers, or the effect such change would have on purely astronomical work, but rather to call the attention of naval officers and nautical men generally to the effect it would have on the various problems of the navigator; whether it will introduce, as Professor Newcomb says, a complicated system for a simple one, and, consequently, cause liability to errors, or whether it will be a gain in simplicity by avoiding the use of the two dates aboard ship, one for the log and one for the navigator. These are questions which the education and experience of naval officers ought to fit them to discuss. If the discussion leads to a general expression of opinion by intelligent navigators that the change will introduce more simple and direct methods, then the opinions of the uneducated navigator, whose opposition Professor Newcomb predicts, ought not to stand in the way of its adoption. It is to be expected that they would object to any change in their habits of computation, no matter for what ultimate gain; if, on the contrary, the change introduces methods which, although simple to educated men, would perplex and confuse those of little mathematical education, these objections ought to be respected.

At the first glance it would seem to be self-evident that the use of two dates to represent the same instant of time, and the necessity of reducing the civil date of the ship's log to the astronomical date of the Nautical Almanac, is not a simple system, and would be attended by liability to error. I think it will be found that navigators of considerable experience have at times made mistakes in taking data from the almanac, by confusing the two dates. Yet, on the other hand, in nearly all the methods of finding a ship's position at sea, or the chronometer error by observations on shore, the hour angle of a celestial object from the upper meridian is either directly the result of the computation, or is used in the computation for finding some other required quantity. Under the proposed system the hour angles obtained would have to be reduced to the lower meridian, or from the lower to the upper meridian to be used in the computation. Whether this reduction would be attended by as much inconvenience and liability to error as the simple process of changing the date, ought to be discussed. It should be considered what changes in the precepts and rules contained in all works on nautical astronomy, and in the tables employed, will be necessary to make perfectly plain to the thumb-rule class the transition to the new system.

A general idea of this can be obtained by an examination in detail of the various problems of navigation. By far the most frequently used are those involving observations of the sun: 1st, for latitude by meridian altitude; 2d, for longitude by time sight; 3d, for latitude by off-meridian sights; and 4th, for latitude by circum-meridian altitudes. Observations of the moon, planets and stars, for latitude or longitude, though comparatively infrequent, are generally used only on those times when observation of the sun has been impossible and it is important to find an approximate position of the ship. On such an occasion, a serious error due to the changed methods might be disastrous. It will first be noted as a factor in every problem that the Greenwich date and time would be indicated simply by the application of the longitude to local date and time; that the latter is

the civil date and time, except that the P. M. hour must be increased by twelve hours. Under the present method, the civil date and time are first reduced to local astronomical date and time, by adding twelve hours to the A. M. time, and decreasing the number indicating the civil date by a unit, for forenoon time; for P. M. time the date and time are shown immediately by the civil date and time. The change substitutes the simple rule that there is but one date, and that is the civil or local date.

The tabulated data for the first problem, finding the latitude by a meridian altitude of the sun, are given on page I. of the Nautical Almanac, for Greenwich apparent noon; for obvious reasons, this could be advantageously left as it is, the navigator bearing in mind only the fact of the coincidence of civil and astronomical time. The use of page I. is confined almost to this one problem, and the data when used for other purposes only required approximately. In all other cases, and perhaps in this, the data would be tabulated, in accordance with the new system, for Greenwich mean midnight, or for times reckoned from that instant.

In the second problem, the data would be taken from page II. of the Nautical Almanac and corrected for the G. M. time of observation; the hour angle would be found from the usual tables; the only change would be in the addition of twelve hours to the hour angle resulting from an afternoon sight, instead of that from the forenoon sight.

In the third problem, the hour angle of the sun would be required for the computation; this would be found for an A. M. sight, by subtracting the L. A. time from twelve hours; for a P. M. sight, by subtracting twelve hours from the L. A. time. Here again a simple change in an old precept is made.

The hour angle required in the fourth problem, to reduce the altitude of the sun to meridian altitude, would be found as before, by comparing the Greenwich time of observation as shown by the chronometer with the Greenwich mean time of apparent noon. The only difference being that noon occurs at 12 hours instead of at 0 hours, as before.

So far as these problems are concerned the changes are very simple; in those involving observations of the moon, planets or stars, the effects of the changes are not so obvious, involving, as they do, changes of hour angle into sidereal time, and the reverse. In time sights of these bodies, the data required from the Nautical Almanac would be found and corrected for the Greenwich mean time of the observation, as before. The resulting hour angle simply designated (in hours, minutes and seconds) as east or west of the meridian, would be reduced to the lower meridian by subtracting it from or adding it to 12 hours, according as the observation was east or west of the meridian. This hour angle would be converted into local sidereal time and this again into local mean time, by the usual formula.

In all other problems in which the hour angle of these bodies is required for the computation, it would be found, as before, by subtracting the right ascension of the body from the local sidereal time; this hour angle, though, is referred to the lower meridian; it would be reduced to upper meridian by subtracting it from 12 hours for an observation east of the meridian, by subtracting 12 hours from the hour angle for an observation west of the meridian.

The following precepts would be found sufficient to make plain the use of the Nautical Almanac and the necessary tables with the origin of the astronomical day at midnight:

I. The astronomical day begins at mean midnight, and coincides with the civil day and date; the hours are counted successively, beginning at midnight from 0 to 24.

II. All data of the Almanac are given for the astronomical date and time as defined in I.

III. The local astronomical time is given directly by the civil time in the forenoon; in the afternoon, by the addition of 12 hours to the civil time.

IV. All hour angles resulting from observations of the moon, planets or stars, expressed simply as east or west hour angle, are to be reduced to the lower meridian by subtracting the east hour angle from 12 hours, or by adding 12 hours to the west hour angle.

V. Hour angles thus reduced are to be reduced to local sidereal time by the addition to the hour angle of the right

ascension of the body for the Greenwich mean time of the observation.

VI. Local sidereal time is reduced to local mean time, as follows: Subtract from the local sidereal time the right ascension of the mean sun for Greenwich mean midnight of the given date plus the correction for the Greenwich mean time; or subtract from the local sidereal time the right ascension of the mean sun for Greenwich mean midnight corrected for longitude; the resulting sidereal interval is corrected as usual by Table II. of the American Ephemeris, or any other table for converting sidereal into mean time interval.

VII. To reduce local mean time to sidereal time, add to the local mean time the right ascension of the mean sun for Greenwich mean midnight of the given date, corrected for the Greenwich mean time.

VIII. To find the hour angle of a body, for use in computation:

1. For the sun: for a forenoon sight, subtract the local apparent time of observation from 12 hours; for an afternoon sight, subtract the local apparent time from 12 hours.

2. For the moon, planet or star: find the local sidereal time by VII.: from the local sidereal time subtract the right ascension of the body observed for the Greenwich mean time of the observation; the result is the hour angle from the lower meridian. For an observation east of the meridian, subtract this result from 12 hours; for an observation west of the meridian, subtract 12 hours from the result.

It will be readily seen that they affect the whole literature of nautical astronomy. The changes, taken by themselves, are not difficult to understand; yet anyone using the existing text-books on the subject would be obliged to make in them the changes necessary to adapt them to the different origin of reckoning time; a difficult thing for one to whom the subject is a new one. Or, studying them as they now are, he would be obliged to change important precepts which had been learned with difficulty. To rightly estimate how difficult this would be, one must look at it not from the standpoint of the skilled navigator, but as one to whom the subject of nautical astronomy is full of perplexities.

It is easy for one who has a thorough knowledge of the theory of the subject to make the necessary precepts for himself, or to readily see the bearing of new ones given for the use of tables under the new system; but would the majority of navigators come under this head?

It may seem trivial and unnecessary to examine in detail all the problems of nautical astronomy to see what effect general changes would produce; yet it serves to show that the meridian of the place is the natural origin to which are referred the various quantities used in or derived from the computations; by this the problems are simple and direct. It is for this reason that the nautical or astronomical day is the most convenient; that it is made to begin at noon. To change its beginning to midnight is only an apparent gain in uniformity. It is not designed for the purpose of chronology, and is not so used; and there is no reason why it should coincide with the day used for that purpose. The civil day is used aboard ship in all cases when it is most suitable; the nautical day only by the navigator in those problems of navigation where it simplifies astronomical calculation. As such, its retention would not conflict with the purposes of those who aim to secure uniformity for chronological and commercial purposes.

Le Yacht of December 19th, 1885, gives a capital summary of the effects of the firing from the Hercules at Portsmouth against the Moncrieff gun. It remarks that the quick-firing and machine guns firing at about 1,300 yards' range for ten minutes made sufficiently close shooting and such hits that it could scarcely be decided whether the gun would have been disabled or not by them. The shrapnel had fired at about 3,000 yards with less effect, and the common shell with still less. None of the dummy detachment were struck. The results were considered strongly in favor of the Moncrieff system. There is an account of the operations of the French fleet in the capture of the Pescadores Islands. The geographical position of the Pescadores is declared to be admirable, better, in fact, than that of Hong-Kong, and the port of Makung, and well suited to the requirements of the French fleet in the Chinese waters.

ON THE RELATIVE CORROSION OF IRON AND STEEL.

By WILLIAM PARKER, Chief Engineer-Surveyor of Lloyd's Register, London.

A FEW weeks ago I had the honor to read a paper before a kindred society—the Institution of Naval Architects—on some peculiarities in the behavior of steel, which had come under my own observation, bearing chiefly upon its qualities of uniformity and ductility. On the present occasion I desire to deal with an entirely different branch of the subject, namely, that relating to the power of steel to withstand corrosion, chiefly in connection with marine boilers and in comparison with iron.

When mild steel was first introduced, a few years ago, two questions were prominently discussed: one was its reliability as a constructive material, the other was its durability so far as corrosion, under different conditions of employment, was concerned. The only experience which had been obtained with steel in these directions, up to that time, was with a different quality of the material to that used in the present day, and it was sufficiently various and contradictory to give rise to much disagreement and no little speculation. This, although it retarded the introduction of mild steel, was in one sense an advantage, for it gave rise to extensive experiments which, while they could not entirely set these questions a rest, at least tended to throw light on the peculiarities of the metal, and hence lead to its more intelligent manipulation and preservation.

Among the earliest important attempts to deal with the question of corrosion in recent years were the investigations of the late Admiralty Boiler Committee. It is not too much to say that the investigations of that committee were eagerly looked for by all interested in the subject. The results have been for some time before the public, and have given rise to much discussion. On the surface they appeared decidedly unfavorable to steel, and deductions have been made from them which are, to my mind, open to exception. I have therefore thought that a brief analysis of the methods adopted in conducting the experiments would be not only interesting, but that, in view of the prominence accorded to those experiments, it is almost called for at the present time.

It will be seen, in looking into the experiments conducted by this committee, that in most of the vertical tubes in which the plates were experimented upon in boiling water (with or without air) there was a copper plate present, and in many instances the different iron and steel plates were held in position by brass or copper rods, while no precautions had been taken to insulate them. Moreover, all the steel plates had been fixed above the iron ones, and as the temperature was greater in the upper than in the lower parts of the tubes, it might have been expected, other things being equal, that the steel would corrode faster than the iron. Fig. 1 shows one of these tubes with the disks attached.

It could not be said, therefore, that the results of the investigations of the boiler committee had settled the question of corrosion, and we were induced to look again to the limited experience that existed, and to the comprehensive experiments made about forty years ago, by Mr. Robert Mallet for the British Association. These experiments were on chilled and "green-sand" cast irons, made by hot and cold blast, or hammered and rolled cast, blister, shear, and spring steels; hammered and rolled wrought iron, including Swedish, Lowmoor, and many ordinary sorts of iron, down to puddled bars.

The value of Mr. Mallet's paper on these experiments is diminished by the fact that the scale was left on the cast iron in nearly every case, while it is not definitely stated whether it was removed from the wrought iron; but the experiments are nevertheless extremely interesting, and a feature worth notice in them is the appended analyses and densities of some of the materials experimented upon. It may be mentioned that the square plates experimented upon were fixed with their four corners to

wooden frames and placed in wooden boxes. The complete results of the experiments will be found in the "Transactions of the British Association" for the years 1838, 1840, and 1843. The losses by corrosion are given in these tables in grains per square inch, per 732 days, and they can be readily compared with the results of some experiments to be described hereafter, as they are nearly a hundred times greater than if they had been given on the scale I have adopted, which is in pounds per square foot per annum.

Comparing the mean losses given by Mr. Mallet of the ordinary sorts of iron with those of Swedish and Lowmoor iron, a difference in favor of the last two is observed when exposed to river-water and to fresh sea-water, while the latter irons compare unfavorably with the common irons when subject to the atmosphere or to what he calls "foul sea-water." The specific gravity stands in close relation to the amount of corrosion, it being found that in all the different classes of material, whether chilled or "green" cast iron, steel, or wrought iron, the heaviest metal of each description lost less than the lightest one. The analyses, of which a full description is appended to the report, show also beyond doubt that the greater the amount of combined carbon (found by combustion), and perhaps silicon, the less was the corrosion. The manganese, which was present in very small quantities, has played such a subordinate part that its effect cannot be traced. These are, so far as I can gather, the principal conclusions to be drawn from Mr. Mallet's paper, although he was of opinion that the finer the quality of the iron the less it will corrode.

These conclusions, if correct, would be an important addition to our knowledge, but they scarcely accord with the results of experience, and, as I have pointed out, we are at once met by the difficulty that in the case of Mr. Mallet's experiments we are left in doubt whether the scale was removed or not; and in the case of the boiler committee, we are left in doubt how far the want of insulation may have affected the results; and there is the further difficulty in comparing the two sets of experiments, that those of Mr. Mallet were only made in low temperatures and those of the boiler committee in high ones.

It was shortly after the issue of the first report of the boiler committee that

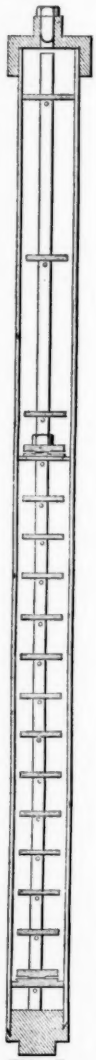


Fig. 1

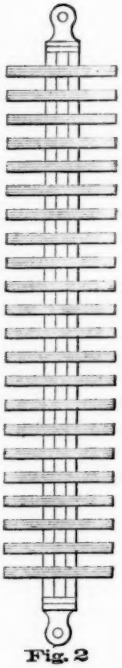


Fig. 2

I commenced to investigate the subject for the committee of Lloyd's Register of Shipping, and I then endeavored to arrange the series of experiments I am

about to describe, so as to avoid as far as possible the sources of error indicated above, and to show the effect of the absence as well as the presence of scale.

In the first place, I obtained twelve disks, 4½ inches diameter and about ¼ inch thick, from each of the following manufacturers:

Common iron from the Parkhead Forge Company, Glasgow.

Common iron from Skerne Iron Works Company, Darlington.

Best quality iron from Lowmoor Iron Company.

Best quality iron from Bowling Iron Company.

Best quality iron from Farnley Iron Company.

Best quality iron from Messrs. Taylor Brothers, Leeds.

Best quality iron from Leeds Forge Company.

Mild steel from Landore Steel Company.

Mild steel from Bolton Iron and Steel Company.

Mild steel from Messrs. John Brown & Co.

Mild steel from Steel Company of Scotland.

Six of the disks from each maker were turned bright entirely to remove all scale, and the other six were turned round the edge only, so as to damage the scale as little as possible. Each disk was carefully weighed to $\frac{1}{10000}$th part of its own weight at the Royal Naval College, Greenwich, through the kindness of Professor Reinold. They were divided into six series, each containing twenty-two disks, one black and one bright, from each of the above-named works, and were fixed together, as shown in Fig. 2, by means of an iron rod which had been covered by a glass tube, the plates being separated from each other by means of glass ferrules about ¾-inch long and 1 inch diameter, thus, so far as possible, insulating each disk and preventing galvanic action being set up between them. I may mention that one of these sets of disks as insulated was immersed in seawater, and it was found that by completing the circuit between any two of them a galvanic current was set up, the bright Lowmoor plate being electro-positive to the whole of the other plates; and as it is impossible that the energy represented by the galvanic current could be generated without oxidation or corrosion of the metal, the necessity of thoroughly insulating the disks is evident.

One series (A) was suspended on the roof of a building in the city of London, exposed to the action of the atmosphere, from the 13th February, 1879, to 13th May, 1880, or 455 days.

Another set (B) was securely fixed under water to the pier at Brighton from 24th February, 1879, to 7th May, 1880, or 437 days.

A third series (C) was so secured to the engine-room floors of a ship trading to the east as to be freely exposed to the action of the bilge water from 9th June, 1879, to 6th February, 1880, or 240 days.

The remaining three sets (D, E, and F) were hung up in the wide waterspaces between the tubes of marine boilers in such a manner that they could not swing about, and were always about twelve inches below the water-line. These boilers were each in different vessels, employed in different trades, and subjected to entirely different treatment. The vessels containing series D belonging to the British India Company, was employed in the East Indian trade; zinc was used in these boilers, and they were blown off as seldom as possible. The immersion lasted from the 15th February, 1879, to 6th June, 1879, and from 14th June, 1879, to 16th February, 1880, or 361 days.

In the steamer containing set E, owned by the Peninsular and Oriental Company, and engaged in the China trade, no zinc was used in the boilers, which were blown out at each terminal port and run up afresh with salt water. This series was exposed from the 15th February, 1879, to 6th June, 1879, and from 12th June, 1879, to 13th November, 1879, or 264 days.

The remaining series (F) was exposed in the boiler of a steam-collier, running between Newcastle and London from the 23d May, 1879, to the 23d April, 1880, or 336 days.

No zinc was used in this boiler, and the water was taken from a point in the Tyne where it is probable that the refuse from one of the local chemical works acidulates it considerably. The boiler was emptied once in ten weeks, and steam was kept up for four days out of every five.

After the completion of the exposure the scale and rust were removed as carefully as possible by scraping and brushing the disks with a file card, and each one was again weighed at the Greenwich College with the same nicety as in the first place, and the results follow in a tabulated form.

Bright Disks.

Table I. gives the loss of metal per square foot per annum. This was obtained by dividing the total loss of weight of each disk by its exact area (about $\frac{1}{4}$ of a square foot) and by the total period during which each set was exposed, which includes the time that the boilers were empty or not in use.

Table II. was compiled from Table I. by dividing each of the losses in the different columns by the loss of the respective Lowmoor plates, so that the different sorts of iron and steel can be readily compared.

Looking at Table II., under the heading of "Cold Water," and in the third column, which gives the mean values of the relative losses in the sea and bilge water, it will be seen that there are but two metals—Bowling iron and Messrs. Brown's steel—which corroded more than Lowmoor iron; and, although the average loss of steel is a little greater than that of iron, the difference is so slight that for practical purposes it is safe to assume that bright steel, exposed to the sea or bilge water, corrodes no faster than bright iron, especially than the better qualities of iron. When exposed to the atmosphere, although there is no great difference between the common and the better sorts of iron, the steel appears to have lost considerably more than either Lowmoor or any other iron; and the same is the case with those disks exposed to the action of boiler-water with or without zinc. But although the absolute losses of both iron and steel is least, the relative difference of losses of steel and iron is greatest in the boiler in which zinc is used. Here the steel has lost about 50 per cent. more than Lowmoor iron, and Lowmoor iron 50 per cent. more than Bowling iron, or 40 per cent. more than the average of the other irons. On referring to Table I., column 4, it will be seen that the average losses per square foot are very small, and that the greatest loss of steel (Messrs. Brown's) in the boiler with zinc was less than the smallest loss of iron (Bowling) in the air, and far less than the Bowling iron in the other two boilers.

Returning to Table II., we observe in column 9 that the steel, with one exception, has on an average corroded only about 14 per cent. more than Lowmoor iron, which has always been considered as amongst the most suitable for the internal portions of boilers, and which has corroded about 20 per cent. more than the ordinary irons. So that, although the present experiments confirm the prevailing impressions that bright, mild steel does corrode faster than iron, when we get from the condition of a marine boiler to cold sea and bilge water, the difference is not so great as to establish the matter beyond question. I have here specimens of the steel and iron disks out of the boiler containing zinc. You will observe that the surface of these specimens is only a little rough, and that the corrosion is very uniform, which is the case with all the bright plates exposed in this boiler.

The plates exposed to the atmosphere are slightly rougher, but also uniform, while those suspended in the boiler of the Peninsular and Oriental Company's vessels are roughly and more irregularly corroded. It is not a little surprising to find that it is only amongst the disks exposed in this boiler, and those exposed to the bilge water that any deep pitting has taken place. There were four cases, of which I produce two specimens (Nos. 3 and 4), which are very distinctly pitted. These were Farnley iron and Messrs. Brown's steel. The one contains a pit mark about $\frac{1}{16}$th of an inch deep, and the others pit marks not quite so deep. The plates submerged in the sea were irregularly attacked and rather patchy. Those exposed to the bilge water looked still worse, the black plates now and again containing pit marks and the fibers of the iron in many cases being distinctly visible. (See samples Nos. 5 and 6.)

The plates exposed in the boiler of the coasting vessel had accidentally come adrift, and the insulation was thus destroyed within the last ten weeks of their immersion, and they do not show the action of the water so distinctly as in the other cases. No pitting has occurred,

but the structure of the iron has in one or two instances been conspicuously brought out. (See sample No. 7.) The Lowmoor disk placed in this boiler also showed very marked projections, one of which, about two square inches in area, contained a thin red scale of what proved by analysis to be copper.

TABLE I.—ABSOLUTE LOSS OF IRON AND STEEL IN POUNDS PER SQUARE FOOT OF BRIGHT SURFACE PER ANNUM.

	Coldwater.		Atmosphere, London.	Boilers.		
	Sea. B.	Bilge. C.	A.	Zinc in boiler. D.	Collier boiler F.	P. & O. boiler. E.
Parkhead common iron	.190	.415	.156	058	.506	.195
Skerne common iron	.137	.556	.151	.062	.485	203
Common Iron mean	163	485	.153	.060	.525	.199
Leeds forge best iron	.168	.475	.169	.061	.609	.164
Taylor's best iron	.198	.527	.155	.056	.657	.191
Bowling best iron	.225	.518	.150	.052	.598	.192
Farnley best iron	.173	.573	.167	.069	.708	.217
Lowmoor best iron	.212	.539	.166	.087	.597	.209
Best iron mean	.195	.526	.161	.067	.633	.194
Landore mild steeel	.208	.480	.206	.120	.666	.234
Brown & Co.'s mild steel	.215	.560	.254	.147	.755	.310
Bolton Co.'s mild steel	.198	.544	.214	.117	.785	.250
Steel Co. of Scotland's mild steel	207	.509	.222	.132	.739	.253
Mild steel mean	.207	.523	.224	.129	.736	.262

NOTE.—A loss of 1 pound per square foot per annum is equal to an average loss of 1-40th inch of thickness per annum.

TABLE II.—COMPARATIVE LOSS OF IRON AND STEEL, TAKING LOSS OF LOWMOOR IRON AS STANDARD.

	Cold water.			Atmosphere, London.	Boilers.				Mean of columns 1, 2, 4, 5, 7 and 8.
	Sea. B.	Bilge. C.	Mean of B. C.	A.	Zinc in boiler. D.	Mean of F. E.	Collier boiler. F.	P. & O. boiler. E.	
Parkhead com'n iron	.90	.77	.83	.94	.67	.94	.95	93	.86
Skerne common iron	.64	1.03	.83	.91	.72	.89	.81	.97	.85
Leeds forge best iron	.79	.88	.83	1.01	.70	.90	1.02	.78	.86
Taylor's best iron	.93	.98	.95	.93	.76	1.00	1.10	.91	.90
Bowling best iron	1.06	.96	1.01	.90	.60	.96	1.00	.92	.91
Farnley best iron	.82	1.03	.94	1.00	.79	1.11	1.19	1.04	.98
Lowmoor best iron	1.00	1.00	1.00	1.00	1.00	1.00	1 00	1.00	1.00
Landore mild steel	.98	.89	.93	1.24	1.38	1.12	1.12	1.12	1.12
Brown's mild steel	1.01	1.04	1.02	1.52	1 69	1.37	1.27	1.48	1 33
Bolton mild steel	.93	1.01	.97	1.28	1.35	1.25	1.31	1.20	1.18
Steel Co. of Scotland mild steel	.97	.94	.95	1.33	1.52	1.22	1.24	1.21	1.20

Table III. shows the results of the analyses of the several bright disks contained in series F.

TABLE III.—ANALYSES AND DENSITIES OF THE RESPECTIVE MATERIALS OF WHICH THE DISKS WERE MADE.

Description of metal.	Manganese.	Carbon-color test.	Sulphur.	Phosphorus.	Silicon.	Copper.	Cobalt and nickel	Density.
Parkhead common iron	trace.	.09	.027	.316	.020	.030	.15	7.618
Skerne common iron	.01	.10	.027	.193	.100	.021	—	7.705
Taylor's best iron	trace.	.12	.005	.136	.013	.00	.05	7.745
Leeds forge best iron	.03	.14	.028	.085	.110	.031	—	7.764
Bowling best iron	trace.	.11	trace.	.101	.100	.016	—	7.791
Farnley best iron	.01	.11	.012	.096	.090	.016	—	7.779
Lowmoor best iron	.01	.10	.022	.142	.120	.022	—	7.689
Landore mild steel	.64	.18	.074	.077	.013	.015	—	7.861
Brown & Co.'s mild steel	.11	.12	.017	.056	trace.	trace.	—	7.854
Bolton & Co.'s mild steel	.52	.19	.068	.041	.060	trace	—	7.849
Steel Co. of Scotland's mild steel.	.26	.10	.035	.057	.032	trace.	—	7.872

NOTE.—The disks analyzed were the bright ones of series F, which, as already stated, was suspended in a boiler supplied with water from a point in the Tyne where it is probably acidulated by the refuse from one of the local copper-works.

A comparison of Tables II. and III., which could trace the relation of the different impurities contained in commercial iron and steel to their corrosion, would be of great interest, but the rate of corrosion has been so varied, even in the same series, and the amount of impurities are so slight, that I have not been able to determine the effect of the different elements.

A comparison of the densities and the corrosion shows the very reverse of what might have been expected from Mr. Mallet's experiments, for though they stand in no relation in series B and C, it appears as if in all others the denser metal has corroded fastest.

The hardening of the plates, by punching the numbers, in no way affected the corrosion of the bright disks, but the glass ferrules protected the part of the disk with which they were in contact when exposed to the atmosphere and bilge water. Curiously enough, however, in some of the cases in the boiler containing zinc, and in the sea, there was found to be considerable extra corrosion in the neighborhood of the glass.

BLACK DISKS.

With the black disks the glass had also protected parts of those disks subjected to the influence of bilge water and the atmosphere, while it had caused strong local corrosion on every one of the disks immersed in the sea and on about half of those exposed in the boiler containing zinc; and perhaps a still more curious circumstance is that throughout the experiments in the boilers the glass ferrules and glass tubes wasted more than the metals, being reduced to about half their original thickness.

The stamping of the numbers, which unavoidably caused some of the scale to come off, had an injurious effect on the plates exposed in the sea and the bilge, although the local corrosion from this cause was only slight in all the other cases.

To enable us to compare the corrosions of the black plates, it is necessary to divide the loss they sustained not by the total surface of the disks, but by the surface of the bare metal only. The percentage of exposed to total surface of each disk was therefore ascertained as nearly as possible, and will be found in Table IV.

A perusal of this table shows that, neglecting Lowmoor iron, which lost almost all its scale, the least scale has come off those disks immersed in the sea, namely, about 30 per cent.; next comes the series D, which was exposed in the boiler containing zinc, with 50 per cent., followed by the two sets exposed to the atmosphere and in the bilges with an average of 70 per cent., while the plates in the boiler of the Peninsular and Oriental steamer lost 95 per cent. Those suspended in the boiler of the coasting ves-

TABLE IV.—RATIO OF UNPROTECTED METALLIC SURFACE OF BLACK DISKS TO TOTAL SURFACE AFTER EXPOSURE.

	Cold Water.		Atmosphere London.	Zinc in boiler.	Hot Water.		Mean of columns 1, 2, 3, 4.
	Sea. B.	Bilge C.	A.	D.	Collier boiler. F.	P. & O. boiler. E.	
Parkhead common iron	.70	.80	1 00	.60	1.00	1.00	.77
Skerne common iron	.30	.50	.90	.50	1.00	.90	.55
Leeds Forge best iron	.45	.85	.85	.80	1.00	1.00	.79
Taylor's best iron	.25	.50	.70	.30	1.00	.90	.44
Bowling best iron	.50	.90	1.00	.70	1.00	.95	.77
Farnley best iron	.30	.80	.95	.80	1.00	1.00	.71
Lowmoor best iron	1.00	1.00	1.00	.95	1.00	.98	.99
Landore mild steel	.30	.60	.50	.35	1.00	.90	.42
Brown's mild steel	.50	.80	.97	1 00	1.00	—	.82
Bolton mild steel	.15	.80	.50	.70	1.00	1.00	.54
Steel Co. of Scotland mild steel	.40	.80	1.00	.70	1.00	1.00	.72
Mean loss of scale due to stamping and exposure, exclusive of Lowmoor	.38	.73	.74	.54	1.00	.96	—

TABLE V.—RATIO OF AVERAGE DEPTH OF CORROSION OF BLACK DISKS TO BRIGHT DISKS.

	Cold Water.		Atmosphere, London.	Zinc in boiler.	Hot Water.	
	Sea. B.	Bilge. C.	A.	D.	Collier boiler. F.	P. & O. boiler. E.
Parkhead common iron	1.2	.9	1.1	1.8	.9	1.0
Skerne common iron	3.3	1.2	.8	2.1	1.0	.5
Leeds Forge best iron	2.4	1.0	.8	1.6	1.0	.8
Taylor's best iron	3.1	1.1	.8	3.1	1.1	.6
Bowling best iron	1.8	.8	1.0	2.5	1.1	.9
Farnley best iron	3.0	.5	.9	1.3	1.0	.9
Lowmoor best iron	.9	.9	1.1	1.3	1.1	.9
Landore mild steel	3.0	.9	.7	1.8	1.2	.9
Brown's mild steel	1.8	.9	1.0	.9	1.0	—
Bolton mild steel	.6	.6	.5	1.0	.9	.9
Steel Co. of Scotland mild steel	2.2	.8	1.1	1.1	1.0	1.1

sel lost all their scale, but this is easily accounted for by the fact that the glass ferrules broke during the last ten weeks of the exposure, thus allowing the disks to rub against each other.

Taking the mean values of the figures in columns 1, 2, 3, and 4, it will be seen that the Lowmoor iron has lost all scale; that Bowling iron, Leeds Forge iron, Parkhead iron, and Brown's steel have lost about 80 per cent.; Farnley Iron and Steel Company of Scotland steel about 70 per cent.; Skerne iron and Bolton steel about 55 per cent.; and Taylor's iron and Landore steel about 40 per cent. It should be mentioned, however, that in stamping the numbers before the experiments, it was observed that the Lowmoor scale appeared to be the softest, and fell off more easily than any other, while the other scales adhered better, but with different degrees of tenacity.

With the help of Table IV., and the accurately ascertained loss of each blank sample, and also the losses per annum per square foot of bright samples as given in Table I., we are able to compare the average depth of corrosion of the bright disks and that of the black disks where the scale has been removed. These results are given in Table V.

Looking at Tables IV. and V., it will be seen that the black disks which lost least scale, have corroded to a greater depth than the corresponding bright disks, and this can only apparently be accounted for by the galvanic action set up. The relative size of the exposed and protected (black scalé) surfaces must have had some influence on the galvanic currents, but they appear to have been considerably affected by the different fluids to which the disks were exposed. If we thus assume that the black disks which corroded to a greater depth than the bright ones were acted upon galvanically, it would appear as if those in series D, which was exposed in the boiler containing zinc, had suffered more from galvanic action than the black disks in the other boilers, while the samples immersed in the sea suffered more than those exposed to the greasy water and moist air of the bilges, although the absolute loss of the respective bright disks was less. There is no practical difference between the loss of the black and the light disks exposed to the atmosphere, and it is evident that here at least no galvanic action could have taken place.

Without knowing how long the scale had been off, it is of course impossible to form a correct measure of the rate at which the black oxide might have been acting galvanically upon the neighboring bare patches, but the comparison is worth making, so far as it goes, and it certainly tends to confirm the belief mentioned by Mr. Barnaby and others, that a rather strong galvanic action does go on between the scale and the patches of bare metal in its neighborhood. Looking to the importance of this point, it is to be hoped that it will be further followed up and scientifically analyzed, as it seems to be from these experiments of quite as much importance as has been attributed to it.

As regards the protection afforded to the disks of series D, suspended in the boiler containing zinc, and, like the others, properly insulated from external galvanic action, the results would seem to indicated that this protection is due, not, as is generally supposed, to the galvanic action of the zinc, but either to the zinc salts, which gradually impregnate the boiling water, and possibly impart to it anti-corrosive properties, or to the action of the zinc, which, as it corrodes, absorbs all corrosive agents contained in the water. It would be interesting to ascertain by experiment what is actually the effect of dissolved zinc salts in boilers.

I have now endeavored to lay before the meeting the results of the series of experiments which it has been my privilege to make. I do not for a moment pretend to be able to account for all peculiarities that have arisen. Some degree of capriciousness was to be expected from those plates on which the scale was left, but I must confess I was hardly prepared to see so much difference between different steels and between different irons under precisely similar conditions as proved to be the case. It would perhaps not be far wrong, speaking generally, to say that the different pieces of iron differed as much among themselves as they did from steel; and certainly the effect produced on my mind, after carefully weighing the results of the experiments, has not been to raise any apprehension that steel boilers or steel ships are likely in the future to corrode to any serious extent more rapidly than iron.

Having made these experiments, however, I desire to say that I do not think undue importance should be attached to any experiments of the kind made on a small scale. The most they can do is to indicate tendencies, and perhaps suggest remedies and precautions earlier than actual experience, which necessarily takes some years to accumulate when a new material is placed under trying conditions. The only real test, however, and the only one that will be finally accepted, is that of practical experience with the boilers themselves.

We have at the present time some 1,100 marine steel boilers running, the majority of which come periodically under the inspection of the engineer-surveyors of Lloyd's Register, and although they have received instructions to give these boilers special attention and to note carefully

any peculiarities they may discover, the accounts I have received from them down to the latest go to show that the steel boilers behave in respect to corrosion about as well as iron boilers. In one or two vessels also where an iron and a steel boiler are working under identical conditions, there is nothing to point to the conclusion that the iron will outlast the steel one. Greater irregularity in the corrosion of the steel is reported, and I am inclined to the belief that this is due, even to a greater extent than shown in the experiments, to the unequal action of the scale, and if it should be found necessary hereafter to remove the scale, the difficulties in the way would not be great, and much of the irregularity and pitting would doubtless be removed. This I know is being done to a limited extent in boilers. The Admiralty also have taken steps to remove the black scale from their ships, and I have heard of at least one firm intending to follow their example. At any rate, I feel sure this meeting will hear with satisfaction that neither from the series of experiments which I have described, nor from our daily experience up to the present time, is there any reason to believe that the question of corrosion is likely to form a bar to the extended use of steel for marine boiler-making purposes. Difficuties may arise, and some unexpected results may be found, but the difficulties will, in my opinion, be easily removed, and the members of this Institute will, I am sure, welcome any one who will lay before them unexpected results, and will assist him in obtaining an explanation of them.

RESULTS OF EXPERIMENTS ON MECHANICAL MOTORS FOR TRAMWAYS MADE BY THE JURY ON RAILWAY APPLIANCES AT THE ANTWERP EXHIBITION.

By Captain DOUGLAS GALTON, D.C.L., C.B., F.R.S.

From the "Journal of the Society of Arts."

An interesting feature of the International Exhibition at Antwerp was the competition which was invited between different forms of mechanical motors on tramways for use in towns, and between different forms of engines for use on light railways in country districts, or as these are termed, *Chemins de Fer Vicinaux*.

These latter have obtained a considerable development in Belgium, Italy, and other Continental States, and are found to be most valuable as a means of cheapening the cost of transit in thinly-peopled districts. But, owing to the fact that the Board of Trade regulations in this country have not recognized a different standard of construction for this class of railway from that adopted on main lines, there has been no opportunity for the construction of such lines in England.

There has, however, been a great development of tramway lines in England, which in populous districts supply a want which railways never could fully respond to; and although hitherto mechanical traction has not attained any very considerable extension, it is quite evident that if tramways are to fulfill their object satisfactorily, it must be by means of mechanical traction.

It is also certain that the mechanical motor which shall be found to be most universally adaptable, that is to say, most pliant in accommodating itself to the various lines and to the varying work of the traffic, will be the form of motor which will eventually carry the day.

The competition between different forms of motors at the Antwerp Exhibition, which was carefully superintended, and which was arranged to be carried on for a reasonable time, so as to enable the qualities and defects of the different motors to be ascertained, affords a starting point from which it will be possible to carry on future investigations.

I have, therefore, thought it advantageous to the interests of the community in this country to bring the results arrived at before this Society; and as the *Chemins de Fer Vicinaux*, to which

one part of the competition was devoted, have no counterpart in this country, it is proposed to limit the present paper to an account of the experiments made on the motors for tramways.

Certain conditions were laid down in the programme published at the opening of the Exhibition, to regulate the competition, in order that the competitors might understand the points which would be taken into account by the judges in awarding the prizes.

The experiments were made upon a line of tramway laid down for the purpose in the City of Antwerp, carried along the boulevards from near the main entrance of the Exhibition to the vicinity of the principal railway station, a distance of 2,292 meters.

The line ended in a triangle of 505 meters, in order that those motors which required to run always in the same direction, should be enabled to do so.

Out of the whole length of the line, viz., 2,797 meters, 2,295 meters were in a straight line, 189 meters in curves of $1\frac{3}{4}$ chain radius, and 313 meters in curves of 1 chain radius. There were, on the line, four passing places, besides a passing place at the terminus; these were joined to the main line by curves of $1\frac{3}{4}$ chain radius.

The line was practically level, the steepest incline being 1 in 1,000; this circumstance is somewhat to be regretted, but the City of Antwerp afforded no convenient locality where a line with steep gradients could have been obtained. The motors were kept in sheds close to the commencement of the line of tramway near the Exhibition, where all necessary cleaning and such minor repairs as were required could take place.

A regular service was established, according to a fixed time-table, to which each motor was required to conform. Each journey was reckoned as starting from the end near the Exhibition, proceeding to the beginning of the triangle, and returning to the starting point. An hour was allowed between the commencement of each journey, fourteen minutes were allowed for a stoppage at the end near the Exhibition, and eighteen minutes at the other end—thus allowing 28 minutes for traveling 2 miles 1,500 yards, or a traveling speed of about 6 miles an hour. The motors were required to work four days out of six, and on one of the four days to draw a supplementary carriage.

An official, assisted by a storekeeper, was appointed to keep a detailed record—

1. Of the work done by each of the motors.
2. Of any delays occurring on the journey, and of the causes of delay.
3. Of the consumption of fuel, both for lighting the fires and for working.
4. Of the consumption of grease.
5. Of the consumption of water.
6. Of all repairs of whatever nature.
7. Of the frequency of cleaning and other necessary operations required for the efficient service of the motor.

The experiments lasted about four months. Five competitors offered themselves, which may be classed as follows: Three were propelled by the direct action of steam, and two were propelled by stored-up force supplied from fixed engines.

PROPELLED BY THE DIRECT ACTION OF THE STEAM.

1. The Krauss locomotive engine, separate from the carriage.
2. The Wilkinson locomotive engine (*i. e.*, Black and Hawthorn), also separate from the carriage.
3. The Rowan engine and carriage combined.

PROPELLED BY STORED-UP FORCE.

4. The Beaumont compressed-air engine.
5. The electric carriage.

It is somewhat to be regretted in the public interest, that other forms of mechanical motors, such as the Mekarski compressed-air engine, or the engine worked with superheated water, or cable tramways, or electrical tramways, were not also presented for competition.

1. The Krauss locomotive is of the general type of a tramway locomotive, but with certain specialties of construction. It has coupled wheels. The weight is suspended on three points. The water-tanks form part of the framing on each side; a covering conceals all except the dome of the boiler. Above the roof is a surface condenser, consisting of 108 copper tubes placed transversely, each of which has an external diameter

of 1.45 inches. The boiler is similar to that of an ordinary locomotive; its axis is 3 ft. 10½ in. above the road. The body of the engine is 9 ft. 11 in. long, and 7 ft. 2½ in. wide. The axles are 4 ft. 11 in. from center to center. The platform extends along each side of the boiler; the door of the fire-box is in the axis of the road. The engine-driver stands on the right-hand side, in the middle of the motor, where he has command of all the appliances for regulating the movements of the engine as well as of the brake.

The Wilkinson (Black and Hawthorn) engine had a vertical boiler and machinery. The cylinders were on the opposite side of the boiler from the door of the fire-box, and mounted independently; the motion of the piston was communicated by means of a crane shaft and toothed wheels to the driving axle. The wheels were coupled. A regulator-injector, and hand-brake were placed at each end, so that the engine driver could always stand in the front, whichever was the direction in which the engine moved; and there was a platform of communication between the two ends, carried along one side of the boiler.

The boiler was constructed with "Field" tubes, the horizontal tube plate having a flue in the middle which carried the heated gases into the chimney.

The visible escape of the steam is prevented by superheating. To effect this the steam, as it leaves the cylinder, passes into a cast-iron chamber adjacent to the boiler, which is intended to retain the water carried off with the steam. From thence the steam passes into a second chamber, suspended at a small height above the grate in the axis of the boiler and of the flue which conveys the heated gases into the chimney, and thence into a sort of pocket enclosed in the last-mentioned chamber, which is open at the bottom, and the upper part of which terminates in a tube passing into the open air. This method of dissipating the steam avoids the necessity of a condenser; but if it be admitted that the steam, in escaping, has a minimum temperature of 572° Fahr., it will carry away 12 per cent. more caloric than would have been required to raise it to a pressure of 150 lbs. per square inch.

The steam escaping through the safety valve is passed through the same apparatus.

The toothed wheel on the driving axle is arranged to act upon another toothed wheel on a shaft connected with the regulator, so as to control its speed automatically.

The length of the engine is 10 ft. 10 in., its width 5 ft. 9 in., and the distance from center to center of the wheels 5 ft. 2 in.

The Rowan tramcar consists of a body 31 ft. long and 7 ft. wide, resting on a two-wheeled bogie behind, and on a four-wheeled bogie in front, this front bogie being the motor, and the whole has the appearance of a long railway carriage, somewhat in the form of an omnibus with a platform at each end, of which the front platform is occupied by the engine. It requires, therefore, either a turntable or a triangle at the end of the line, so as to enable it to reverse its direction.

This motor is a steam-engine of light and simple form, supplied with steam from a water-tube boiler with very perfect combustion, so that no smoke escapes. The boiler is somewhat on the principle of a Shand and Mason boiler; it is so built that it can easily be opened and every part of the interior examined and cleaned.

The peculiarity of the Rowan motor is the simplicity of the attachment of the engine to the carriage, and the facility with which it can be detached when required for cleaning or repair, viz., in five or six minutes.

The steam can be got up in the engine with great rapidity if a change of engine is required. When, however, the engine is detached, the carriage loses its support in front and is therefore not serviceable. When necessary, the combined motor can draw a second ordinary carriage.

The motor, by itself, occupies a length of 9 ft. 8 in. It has two horizontal cylinders; the four wheels of the bogie are coupled, and between the wheels the sides of the framing are rounded to allow two vertical boilers to stand. These boilers have vertical tubes for the water, which are joined together at the top by a horizontal cylinder. Each boiler, with its covering, is 1 ft. 9 in. in diameter. The boilers stand 1 ft. 9 in. apart, thus affording space between them for the mo-

tive machinery, including the pump. The crank axle is behind the boilers. The levers, the injector, the access to the fire-box, a pedal for working the engine-brake, as well as a screw brake for the carriage, are all in front. The brakes act on all six wheels, are worked by the driver, and the whole weight of the engine, car, and passengers being carried on these wheels, the car can be stopped almost instantaneously; and as over two-thirds of the entire weight of the car and passengers rests on the four driving wheels, there is always sufficient adhesion on all reasonable inclines, and the adhesion is augmented as the number of passengers carried increases. Hence, this car is adapted for lines with heavy grades.

A small water-tank is attached to the framing; two small boxes for coal or coke, with a cublic capacity of about 3½ ft., are attached to the plate in front of the bogie. The covering of the boilers is in two parts, which are put on from each side horizontally, and screwed together in the center. The removal of the upper part enables the tubes to be examined and cleaned. The draught is natural; the base of the chimney is 3 ft. 2 in. from the grate; the height of the chimney is 5 ft. 2 in.

The steam from the cylinders passes directly into a condenser placed on the top of the carriage. The condenser is made of corrugated copper sheets a millimeter thick. Two sheets, about 15 to 18 in. wide and 15 ft. long, are laid together and firmly soldered, forming a chamber. Twenty of these chambers are placed side by side on the top of the carriage, connected with a tube at each end, so as to allow the steam to pass freely through them. The lower corrugations in the several chambers are connected together, and thence a pipe with a siphon to stop the steam is carried to a water tank under the carriage, which thus receives the condensed water. This arrangement afforded a condensing surface of about 800 sq. ft. It should be mentioned that, with larger engines, Mr. Rowan employs as much as 1,600 ft. of condensing surface. The nearness of the chambers to each other tends, no doubt, to diminish the power of condensing the steam, but this is somewhat compensated by the artificial circulation of air produced by the movement of the carriage. But, in any case, if there is surplus steam, the pipe from the condenser causes it to pass under the grate, whence it rises superheated and invisible through the fire and up the chimney.

Under the carriage, attached to the framing, are four reservoirs, holding about 3½ cubic feet of water, of which water space one-half acts as a reservoir for cold feed-water, and half for the condensed water. A tube from the small reservoir on the engine communicates through valves with the reservoirs of hot and cold water on the carriage.

The consumption of cold water measured during two days was 2.86 lbs. per kilometer; assuming that the boiler evaporated 6.5 lbs. of water per pound of coal, the cold water formed one-fifth of the total feed-water required.

The carriage, *i. e.*, the part occupied by passengers, is 21 ft. 3 in. in length. It holds seats for forty-five passengers, besides those who would stand on the gangway and platform. The seats are placed transversely on each side of a central corridor, each seat holding two people. The platform of the carriage is about 2 ft. 6 in. above the rails. Passengers have access to the interior from behind by means of the end platform, and in front, near the engine, from the two sides. As already mentioned, the hind part of the carriage rests upon two wheels, the front part being, as already mentioned, supported on the engine bogie. To effect this support, the hinder part of the framing of the engine is formed in a half circle, with a broad groove, in which the ends of two springs are arranged to slide. The centers of the springs form the support of the framing of the carriage.

The framing of the engine bogie is attached to the hind bogie truck of the carriage by two diagonal drawbars. The coupling is effected by bolts close to the engine, and the car is drawn entirely by means of the bogie pin of the hind bogie. The trucks are 16.5 ft. apart.

Table I. (p. 205) shows the dimensions of different parts of these three steam motors, as well as their weights.

The Beaumont engine, worked by compressed air, may be generally said to be similar to that described in a paper read before the Society of Arts on the 16th

TABLE I.

		Krauss.	Wilkinson.	Rowan.
Diameter of cylinder	d	5.5 in.	6.5 in.	5.1 in.
Length of stroke	l	11.8 in.	9 in.	9.8 in
Diameter of wheels	D	31.5 in.	27.5 in.	29.5 in.
Pressure at which boiler is worked	p	220 lbs.	147 lbs.	191 lbs.
$\frac{p d^2 l}{2.D}$	E	1,210 lbs.	1,509 lbs.	805 lbs.
Total heating surface	S	105 sq. ft.	105 sq. ft.	64 sq. ft.
Grate surface	G	2.7 sq. ft.	5.4 sq. ft.	3.1 sq. ft.
Surface of condenser	C	274.482 sq. ft.	None.	861.120 sq. ft.
Weight in running order (motor only)	P′	15,400 lbs.	15,400 lbs.	9,020 lbs.
" " " (total)	P″	..	..	15,400 lbs.
Contents of water tank	..	28.24 cu. ft.	13 cu. ft.	4.2 cu. ft.
" coal bunks	..	14.12 cu. ft.	12.5 cu. ft.	3.5 cu. ft.
	$\frac{P'}{E}$	12.7 lbs.	10.2 lbs.	11.2 lbs.
	$\frac{P''}{E}$	..	..	19.125 lbs.
	$\frac{P'}{S}$	146	147	140
	$\frac{P'}{G}$	5,722	2,855	2,839
	$\frac{C}{S}$	2.6	..	13.4
	$\frac{S}{G}$	102	..	275

of March, 1881, to which, however, some improvements have been since introduced.

The apparatus for compressing the air was placed in the shed. The air was compressed to 63 atmospheres by a pump worked by a steam-engine, and stored in cylindrical reservoirs of wrought iron without rivets. A pipe led the air from the reservoirs to the head of the tramway, where the cylinder placed on the motor for storing the air during the journey could be conveniently charged.

The air was compressed by means of four pumps, placed two and two in a water-box, and worked by the direct action of a compound engine with cylinders placed in juxtaposition, of 8 in. and 14 in. diameter respectively, with an equal length of stroke of 13 in.

The air, after being forced through the first pump cylinder, passed successively through the other three, the diameters of which were of proportionately decreasing sizes, viz.: 8.2 in., 5 in., 3.5 in., and 2 in., and the air, on leaving each cylinder, passed on its way to the next cylinder through a coiled pipe immersed in flowing water to remove the heat generated. This cooling surface amounted to nearly 54 sq. ft.

The cooling of the air was very efficient. In an experiment made on this question, the temperature of the compressor did not vary to the extent of 9° Fahr. in charging the reservoir from 40 to 63 atmospheres, occupying an hour and a-half, the consumption of water during the time being about 1,400 gallons.

The fixed reservoirs were of about 240 cu. ft. capacity.

The motor formed part of a compound vehicle, which may be said to have consisted of two parts joined together by an articulated corridor, the whole being covered by a roof which was approached from the platform behind by an easy staircase. On this roof were seats for outside passengers.

The front part of the compound vehicle contained the motor, as well as a compartment for six inside passengers, with roof space for twenty passengers, and weighed about 15,400 pounds when empty; the hind part contained accom-

modation inside for twelve passengers, and outside for fourteen passengers, and weighed 6,600 lbs.

The combined vehicle was entered from the platform in the rear, which could hold four passengers, and from thence, as already mentioned, the staircase led on to the roof. The total number of passengers this vehicle could accommodate was thus—eighteen inside, thirty-four on the roof, four on the platform, or, fifty-six in all.

The total length of the carriage was 29 ft. 7 in., width 7 ft. The distance between the axes of the bogies was 16 ft. 9 in. The distances apart of the centers of the wheels were, in the case of the hind bogie, 3 ft. 9 in., and in the case of the front bogie, 4 ft. 4.6 in.

The motor is a compound engine, the diameters of the cylinders being 4.9 in., and 1.9 in., with 12 in. stroke. The diameter of the wheels was 2 ft. 4 in. A small boiler is placed on one side, in front, for creating steam, which passes into a steam-jacket, enclosing the pipe of communication from the reservoir to the cylinders, as well as the cylinders themselves, so that the air was warmed before it escaped. The reservoirs on the motor contained 71 cu. ft.

In an experiment made on charging the reservoir in the motor, the pressure in the fixed reservoirs, at the time of charging the reservoirs on the motor, was 63.8 atmospheres, at a temperature of 68° Fahr. One atmosphere was lost by letting the air into the pipe laid between the shed and the tramway where the motor stood; when the reservoir on the motor was charged, the pressure fell to 42.6 atmospheres in the fixed reservoirs, at a temperature of 55° Fahr.

The pressure in the reservoir on the motor, when ready to start, was 42.6 atmospheres, at a temperature of 84° Fahr. On its return at the end of forty-six minutes, after a journey, as above mentioned, of about 3¼ miles, including the triangle, the pressure had fallen to 20.9 atmospheres, and the temperature to 71° Fahr. The weight of air used during the journey was thus about 110 lbs., or, say, 34 lbs. per mile. The coal consumed by the stationary engine to compress the air amounted to 39 lbs. per mile, in addition to 3 lbs. of coke per mile for warming the exhaust.

Whilst the motor was performing its journey, the stationary steam-engine was employed in raising the pressure in the fixed cylinders to 63 atmospheres, and worked, on an average, during fifty minutes in each hour; during the rest of the journey it remained idle. It was thus always employed in doing work in excess of the pressure which could be utilized on the car, and the work was, under the circumstances of the case, necessarily intermittent. This was a very unfavorable condition of working.

In the electric tram-car the haulage was effected by means of accumulators. The car was of the ordinary type, with two platforms. It was said to have been running as an ordinary tramcar since 1876. It had been altered in 1884 by raising the body about six inches, so as to lift it clear of the wheels, in order to allow the space under the seats to be available for receiving the accumulators, which consisted of Faure batteries of a modified construction. The accumulators employed were of an improved kind, devised by M. Julien, the under manager of the Compagnie l'Electrique, which undertook the work.

The principal modification consists in the substitution, for the lead core of the plates, of one composed of a new unalterable metal. By this change the resistance is considerably diminished, the electromotive force rises to 2.40 volts, the return is greater, the output more constant, and the weight is considerably reduced. The plates being no longer subject to deformation, have the prospect of lasting indefinitely. The accumulators used were constructed in August, 1884.

The car, as altered, had been running as an electric tramcar on the Brussels tramways since October, 1884, till it was transferred to the experimental tramway at Antwerp. The accumulators had been in use upon the car during the whole of this period, and they were in good order at the end of the experiments, that is to say, when the Exhibition closed at the end of October, 1885.

The accumulator had forty elements, divided into four series, each series communicating by means of wires fixed to the floor of the car, with commutators which connected them with the dynamo used as a motor.

There were two sets of these batteries, or accumulators, one of which was being charged in the shed whilst the other was in use. The exchange required ten minutes, including the time for the car to go off the tramway into the shed and return to the tramway. This exchange took place after every seven journeys. Therefore, the two batteries would have sufficed for working the car over a distance of about 42 miles during 16 hours.

It may be observed that the first service in the morning would be performed by means of the accumulators charged during the afternoon and evening of the previous day.

Each element of a battery was composed of 19 plates, of which 9 were positive, 4 millimeters thick, and 10 negative, 3 millimeters thick. Each positive plate weighed 1.44 lbs., of which about 25 per cent. consisted of active material. Each negative plate weighed nearly 1 lb., of which one-third consisted of active matter. The weight of the metallic part of the battery amounted, therefore, to 1,846 lbs.; and the whole battery, including the case and the liquid, amounted to 2,464 lbs., which contained 499 lbs. of active matter, or 20.25 per cent. The four cases in which the battery was contained were so arranged as to divide the weight equally between the wheels.

Two commutators enclosed in a box were placed on the platforms at the two ends of the carriage, so as to be available for moving in either direction.

The accumulators were divided into four series of ten double elements, which, by means of the commutators, could be united under four combinations, viz.:

1st.	4 series in quantity	—1	in tension.
2d.	2 " " "	2	"
3d.	.....	3	"
4th.	....	4	"

Finally, a fifth movement united the four series in quantity, coupling them on each other, and putting the dynamo out of circuit, thus restoring equilibrium. When in a state of repose the handle was so arranged as to keep this latter switch turned on. The accumulators were arranged for charging in two series united in quantity, each containing twenty double elements. The charge was effected by a Gramme machine, worked by a portable engine. Each of these series received its charge during seven hours for the ordinary service of the car, and during nine hours for the accelerated service.

The accumulators on the car actuated a Siemens dynamo, acting as a motor, such as is used for lighting, having a normal speed of 1,000 revolutions, fixed on the frame of the carriage. The motion was conveyed from the pulley on the dynamo by means of a belt passing round a shaft fixed on movable bearings to regulate its tension, and thence to the axles by means of a flat chain of phosphor-bronze. The chain was adopted as the means of moving the axle, on account of its simplicity and facility of repair by unskilled labor.

The speed was fixed at 4 meters per second (which corresponds with a speed of nearly 9 miles per hour), for 1,000 revolutions of the dynamo, and it was regulated by cutting a certain number of the accumulators out of circuit, instead of by the device of inserting resistances which cause a waste of energy. By breaking the circuit entirely the motive power ceased, and the vehicle might either be stopped by the brakes or allowed to run forward by gravity, if the road were sufficiently inclined. The reversal of the motor was effected by means of a lever which reversed the position of the brushes of the dynamo.

The dynamo could be set in motion, and the carriage worked from either end, as desired. The handle to effect this was movable, and as there was only one handle and this one was in charge of the conductor, he used it at either end as required.

It should be mentioned that the car was lighted at night by two incandescent lamps, which absorbed 1.5 amperes each; and the brakes also were worked by the accumulators.

The weight of the tramcar was 5,654 lbs.; the weight of the accumulators was 2,460 lbs.; the weight of the machinery, including dynamo, 1,232 lbs. The car contained room for fourteen persons inside and twenty outside. Under the conditions of the competition the car was required to draw a second car occasionally.

The jury made special observations upon the work required to move the car between the 20th September and 15th October, 1885. Seals were attached to the accumulators. Moreover, from the

27th of September, after each charge, seals were placed on the belts from the steam-engine to prevent any movement of the Gramme machine, so that there could be no charges put into the accumulators beyond those measured by the jury.

The instruments used for measuring were Ayrton's ampere meter and Deprez's volt meter, which had been tested in the Exhibition by the Commission for Experiments on Electrical Instruments, under the presidency of Professor Rousseau. Besides this, Siemens electro-dynamometer and Ayrton's voltmeter were used to check the results; but there was no practical difference discovered. During the period of charging the accumulators, the intensity of the current and the electromotive force was measured every quarter of an hour, and thence the energy stored up in the battery was deduced. It may be mentioned that the charge in the accumulators, when the experiments were commenced, was equal in amount to that at their termination.

An experiment was made on 21st October to ascertain, as a practical question, what was the work absorbed by the Gramme machine in charging the accumulators. The work transmitted from the steam-engine was measured every quarter of an hour by a Siemens dynamometer, at the same time the intensity of the electromotive force given out by the machine, as well as the number of the revolutions it was making, was noted. It resulted that for a mean development of 4 mechanical horse-power, the dynamometer gave into the accumulators to be stored up 2.28 electrical horse-power, or 57 per cent. The intensity varied between 25.03 and 23.51 amperes during the whole time of charging. Of this amount stored up in the accumulators a further loss took place in working the motor; so that from 30 to 40 per cent. of the work originally given out by the steam-engine must be taken as the utmost useful effect on the rail.

It was estimated that to draw the carriage on the level .714 horse-power was required, or, if a second carriage was attached, .848 horse-power would draw the two together. This would mean that, say, 2 horse-power on the fixed engine would be employed to create the electricity, for producing the energy required to draw the carriage on the level.

The electric tramcar was quite equal in speed to those driven by steam or compressed air, and was characterized by its noiselessness and by the care with which it was manipulated.

Assuming the car, by itself, cost the same as an ordinary tramcar, the extra cost relatively to other systems was stated as being, according to the following figures, viz.: The Gramme machine cost £48, the motor £208, and the accumulators 2.25 francs per kilogramme (10d. per pound). To these must be added the cost of erection, and of switches for manipulating the current; as well as the proportion of the cost of a fixed engine to create the electricity.

Having thus given a general description of the various motors which were presented for competition, I will now give a brief summary of some of the principal particulars obtained during the competition. In the first place, it may be mentioned that the Jury consisted of the following:

President.—M. Hubert, Ingénieur en Chef, Inspecteur de Direction à l'administration des chemins de fer de l'Etat Belge.

Vice-President.—M. Beliard, Ingénieur des Arts et Manufactures, délégué par le Gouvernement Francais.

Members.—MM. Douglas Dalton, Capitaine du Génie, délégué par le Gouvernement Anglais; Gunther, Ingénieur Commissaire Général de la Section Allemande à l' Exposition d'Anvers; Hubert, Ingénieur à l'administration des chemins de fer de l'Etat Belge, Professeur à la Université de Bruxelles; Dery, Ingénieur Chef de service a l'administration des chemins de fer de l'Etat Belge.

Secretary.—M. Dupuich. Ingénieur Chef du service du matérial et de la traction à la Société Générale des chemins de fer économiques.

Reporter.—M. Belleroche, Ingénieur en Chef, a la traction et au matérial des chemins de fer du Grand Central.

Members added by the Jury.—MM. Vinçotte, Ingénieur, Directeur de l' Association pour la surveillance des machines a vapeur; Laurent, Ingénieur des mines et de l'Institut électro-technique de l'Universite de Liége.

The original programme of the condi-

tions which were laid down in the invitation to competitors, as those upon which the adjudication of merit would be awarded, contained twenty heads, to each of which a certain value was to be attached; and, in addition to these special heads, there were also to be weighed the following general considerations, viz:

a. The defects or inconveniences established in the course of the trials.

b. The necessity or otherwise of turning the motor, or the carriage with motor, at the termini.

c. Whether one or two men would be required for the management of the engine.

As regards these preliminary special points, the compressed-air motor, as well as the Rowan engine, required to be turned for the return journey, whereas the other motors could run in either direction.

In regard to this, the electric car was peculiarly manageable, as it moved in either direction, and the handle by which it was managed was always in front, close to the brake. This carriage was the only one which was entirely free from the necessity of attending to the fire during the progress of the journey, for even the compressed-air engine had its small furnace and boiler for heating the air.

Each of the motors under trial was managed by one man.

The several conditions of the programme may be conveniently classified in three groups, under the letters A, B, C. Under the letter A have been classed accessory considerations, such as those of safety and of police. These are of special importance in towns. But their relative importance varies somewhat with the habits of the people as well as with the requirements of the authorities; for instance, in one locality or country conditions are not objected to, which in another locality are considered entirely prohibitory.

The conditions under this head are:

1. Absence of steam.
2. Absence of smoke and cinders.
3. Absence, more or less complete, of noise.
4. Elegance of aspect.
5. The facility with which the motor can be separated from the carriage itself.
6. Capacity of the brake for acting upon the greatest possible number of wheels of the vehicle or vehicles.
7. The degree to which the outside covering of the motor conceals the machinery from the public, whilst allowing it to be visible and accessible in all parts to the engineer.
8. The facility of communication between the engineer and the conductor of the train.

In deciding upon the relative merits of the several motors, so far as the eight points included under this heading are concerned, it is clear that, except possibly as regards absence of noise, the electrical car surpassed all the others.

The compressed-air car followed, in its superiority in respect of the first three points, viz., absence of steam, absence of smoke, and absence of noise; but the Rowan was considered superior in respect of the other points included in this class.

Under the letter B have been classed considerations of maintenance and construction.

9. Protection, more or less complete, of the machinery against the action of dust and mud.
10. Regularity and smoothness of motion.
11. Capacity for passing over curves of small radius.
12. The simplest and most rational construction.
13. Facility for inspecting and cleaning the interior of the boilers.
14. Dead weight of the train compared with the number of places.
15. Effective power of traction when the carriages are completely full.
16. Rapidity with which the motor can be taken out of the shed and made ready for running.
17. The longest daily service without stops other than those compatible with the requirements of the service.
18. Cost of maintenance per kilometer. (It was assumed, for the purposes of this sub-heading, that the motor or carriage which gave the best results under the conditions relating to paragraphs 9, 10, 12, and 13, would be least costly for repairs.)

As regards the first of these, viz., protection of the machinery against dirt, the machinery of the electrical car had no

protection. It was not found in the experiments at Antwerp that inconvenience resulted from this; but it is a question whether in very dusty localities, and especially in a locality where there is metallic dust, the absence of protection might not entail serious difficulties, and even cause the destruction of parts of the machinery.

In respect of the smoothness of motion and facility of passing curves, the cars did not present very material differences, except that the cars in which the motor formed part of the car had the preference.

In the case of simplicity of construction, it is evident that the simplest and most rational construction is that of a car which depends on itself for its movement, which can move in either direction with equal facility, which can be applied to any existing tramway without expense for altering the road, and the use of which will not throw out of employment vehicles already used on the lines; the electric car fulfilled this condition best, as also the condition numbered 13, as it possessed no boiler.

In respect to No. 14, viz., the ratio of the dead weight of the train to passengers, if we assume 154 lbs. as the average weight per passenger, the following is the result in respect of the three cars in which the power formed part of the car:

$$\text{Electric car} \ldots\ldots \frac{9{,}350 \text{ lbs.}}{154 \times 34} = 1.78.$$

$$\text{Rowan} \ldots\ldots\ldots \frac{15{,}950 \text{ lbs.}}{154 \times 45} = 2\,30.$$

$$\text{Compressed air} \ldots \frac{22{,}000 \text{ lbs.}}{154 \times 56} = 2.55.$$

The detached engines gave, of course, less favorable results under this head.

Under head No. 15 the tractive power of all the motors was sufficient during the trials, but the line was practically level, therefore, this question could only be resolved theoretically, so far as these trials were concerned, and the table before given affords all the necessary data for the theoretical calculation.

As regards the rapidity with which the motors could be brought into use from standing empty in the shed, the electric car could receive its accumulators more rapidly than could the boiler for heating the exhaust of the compressed-air car be brought into use.

As regards the steam motors, the following were the results from the time of lighting the fires:

The Rowan—
In 34 minutes.....3 atmospheres.
36 " .....4 "
(At this pressure the vehicle could move.)
40 " .....8 "

The Wilkinson—
In 35 minutes.....2 atmospheres.
40 " .....4 "
44 " .....6 "
47 " .....8 "

The Krauss machine required two hours to give 6 atmospheres, which was the lowest pressure at which it could be worked.

The results under No. 17, viz., the fewest interruptions to the daily service, class the motors in the following order—Krauss, electric, Rowan, Wilkinson, compressed air. The chief cause of injury to the compressed-air motor arose from the carelessness of the drivers, who allowed the steam boiler to be burnt out. Unfortunately, these drivers were new to the work.

Under the letter C are classed considerations of economy in the consumption of materials used for generating the power necessary for working.

19. Minimum consumption of fuel (either coke or coal), in proportion to the number of kilometers run, and to the number of places, assuming for the seats a width of at least sixteen inches for each person seated.

It must be borne in mind that the conditions of the competition required that a second car should be periodically drawn by the motor, and that the calculations which follow include the total number of

TABLE II.

Description of motor.	Total number of train-miles run.	Total consumption of fuel.	No. of lbs. per train-mile.
		lbs.	
Electric.....	2,358.9	14,786.	6.16
Rowan.....	2,616.9	14,498.	5.42
Wilkinson....	2,473.3	22,000.	8.82
Krauss.....	2,457.8	22,726.	9.10
Compressed air	2,259.1	90,420.	39.48

miles run, the total amount of fuel, etc., consumed; and the total number of passengers which could be conveyed by each moter, during the total time that the experiments were being carried on.

TABLE III.

Description of motor.	No. of places indicated on the cars, per mile run.	Consumption of fuel.	No. of lbs. of fuel consumed per places indicated per mile run.
		lbs.	
Electric.....	80,203.5	14,786.	.18
Rowan.....	148,399.6	14,498.	.09
Wilkinson....	119,085.1	22,000.	.18
Krauss.....	108,983.9	22,726.	.20
Compressed air	128,189.3	90,429.	.69

TABLE IV.

Description of motor.	No. of seats per mile run.	Consumption of fuel.	No. of lbs. of fuel consumed per seat per mile run.
		lbs	
Electric.....	61,591.2	14,786.	.23
Rowan.....	135,928.8	14,498.	.10
Wilkinson....	93,965.6	22,000.	.23
Krauss.....	86,039.9	22,726.	.25
Compressed air	132,732.7	90,420.	.66

As regards the figures in these tables, it is to be observed that the consumption of fuel for the electric car is, to a certain extent, an estimate; because the engine which furnished the electricity to the motor also supplied electricity for electric lights, as well as for an experimental electric motor which was running on the lines of tramway, but was not brought into competition.

20. Minimum consumption of oil, of grease, tallow, etc. (the same conditions as in No. 19.)

TABLE V.

Description of motor.	Total number of miles run.	Total consumption of oil, tallow, etc.	Consumption of oil, tallow, etc., per train-mile run.
		lbs	
Electric.....	2,358.9	99.0	.038
Rowan	2,616.9	106.7	.038
Krauss.....	2,457.8	188.5	.073
Wilkinson....	2,473.3	255 4	.101
Compressed air	2,259.1	585.2	.255

In addition to these considerations, it was thought useful to investigate the quantity of water consumed in the case of these engines which used steam. The experiments made on this point showed as the consumption of water—

	Gallons per mile.
Rowan.....	.75
Compressed air.....	1.06
Wilkinson.....	5.89
Krauss	6.52

Thus, owing to the large proportion of water returned from the condenser to the tanks, the Rowan actually used less water than the compressed-air engine.

CONCLUSION.

The general conclusion to which these experiments bring us is that, undoubtedly, if it could certainly be relied upon, the electric car would be the preferable form of tramway motor in towns, because it is simply a self-contained ordinary tramcar, and in a town the service requires a number of separate cars, occupying as small a space each as is compatible with accommodating the passengers, and which follow each other at rapid intervals.

But the practicability and the economy of a system of electric tramcars has yet to be proved; for the experiments at Antwerp, whilst they show the perfection of the electric car as a means of conveyance, have not yet finally determined all the questions which arise in the consideration of the subject. For instance, with regard to economy, the engine employed to generate the electricity was not in thoroughly good order, and from its being used to do other work than charg

ing the accumulators of the tramcar, the consumption of fuel had to be to some extent estimated. In the next place, the durability of the accumulators is still to be ascertained; upon this much of the economy would depend. And in addition to this question, there is also that of the durability of parts of the machinery if exposed to dust and mud.

After the electric car, there is no question but that, at the Antwerp Exhibition, the most taking of the tramway motors was the Rowan, which was very economical in fuel, quite free from the appearance of steam, and very convenient and manageable.

The economy of the Rowan motor arises in a large degree from the extent of its condensing power, by means of which a considerable supply of warm water is constantly supplied for use in the boiler, and, consequently, the quantity of water which has to be carried is lessened, and the fuel is economized.

Independently, however, of its convenience as a motor for tramways in towns, the Rowan machine has been adapted on the Continent to the conveyance of goods as well as passenger traffic on light branch railways, and fitted to pass over curves of 50 ft. radius, and up gradients of 1 : 10.

In England, with our depressed trade and agriculture, there is a great want in many parts of the country of a cheap means of conveyance from the railway stations into the surrounding districts; such a means of conveyance might be afforded by light railways along or near the roadside, the cost of which would be comparatively small, provided that the expensive methods of construction, of signaling, and of working, which have been required for main lines, and which are perfectly unnecessary for such light railways, were dispensed with.

It is certain that this question will acquire prominence as soon as a system of local government has been adopted, in which the wants of the several communities have full opportunity of asserting themselves, and in which each local authority shall have power to decide on those measures which are essential to the development of the resources of its own district, without interference from a centralized bureaucracy.

Discussion.

Mr. Preece, F. R. S., said he was a juryman at the Antwerp Exhibition, and though his official duties did not require him to inspect and report upon these tramcars, he took every possible opportunity of examining them, and being an electrician, of traveling backwards and forwards on the electric car. He was delighted to find that the anticipations he formed, at an early period of the Exhibition, had been fully borne out by what Captain Galton had said. At the same time it was rather difficult to discover in what respect this particular car differed from others of a similar kind which had been experimented on in England. The chief advantage it had, seemed to him to be that it had been examined and reported on by Captain Douglas Galton. One very similar ran backwards and forwards for some time in the works of the Electric Power Storage Company at Millwall, and one designed by Mr. Reckenzaun had been running some time in Berlin. He tried to find out the special features of the accumulator used by M. Julien, but failed to do so. It was said to be formed on a framework of some unalterable metal, but he did not discover what it was; he heard by a side-wind that it was nickel, but at any rate, as far as the performance and appearance went, he could see no difference between it and those used in England. At different times, he had inspected and ridden on various electric tramcars, some driven direct by the current, like that at Portrush, or the one at Cleveland, United States, the only difference in these two being that the current in Ireland was of low tension, and that in the United States was of high tension. There was also one, which he had not seen, at Blackpool. As an electrician, he had seen very great difficulties in carrying out a system of tramways worked by electricity, but, like all other difficulties, experience and study succeeded in removing them, and he had the greatest confidence in the future use of electricity for this purpose, whether applied directly or by the aid of accumulators. There was a great want of confidence in this latter system, the reason for which was not hard to discover, because they were thrust on the world long before either it was ready to receive them or they were properly developed. One

of the first places in which the tide began to turn was in the Society of Arts. Some time back they had succeeded in lighting the hall by means of accumulators, and all present could see with what absolute steadiness the light was maintained. Captain Galton, at the end of the paper, said that undoubtedly, if it could be relied on, electricity would be the best motor for tramways; and that was the way in which everyone spoke of it. But there was no power on earth used for the service of man which could be so certainly relied on as electricity. These doubts were due to the fact that the early applications of electricity were so striking in their character, such, for instance, as reproducing speech by the telephone, that they at once caught the attention of the public, who at once believed the thing was perfection before it had really reached the practical stage. So it was with the accumulator; the idea of storing up a horse-power in a deal box appealed to the public imagination, and when the fact was first published in the *Times*, that a certain gentleman brought such a box from Paris to Sir William Thomson, in Edinburgh, inordinate expectations were raised; then, when it was not found at first a practical instrument, they began to use such expressions as he had referred to, and thought it could not be relied on. He had no doubt, however, we should yet live to see in London and the suburbs, the Antwerp experiments repeated on a much larger scale.

Mr. A. Reckenzaun said this was one of the most interesting and practical papers ever read on the subject of the mechanical propulsion of tramcars, but it would have been still more useful had the experiments been carried out on a line presenting more difficulties. It was very easy to travel on a level line, but very difficult to travel on an incline. For instance, if it required 2 horse-power on the level, it would take 4 horse-power to go at the same speed up a gradient of 1 in 75, 6 horse-power for a gradient of 1 in 37, and 8 horse-power for 1 in 25, which was not very serious after all. Again, the sharpest curve on the line at Antwerp was only of a radius of one chain, and he was rather surprised it was not made sharper. He had recently been running a car in Berlin, where he had received much more encouragement than in London, and he believed a fair amount of business would be done there before any start was made in England at all. There, one curve was 11 meters radius, and there were several of 15 meters. In the Antwerp tram it appeared there was a certain amount of noise caused by the chain gear, and this kind of gearing was not very practical in the case of a tramway where there was a deal of dirt to guard against. He had adopted a worm wheel, such as was shown on a large drawing of his tramcar on the wall. Many experienced engineers advised him not to use this gearing, but having arranged it so that the wheel dipped into an oil bath, and the whole boxed in, so that little or no dirt could find access, he found that there was not nearly so much loss by friction as had been stated, previous experiments having all been made apparently at low speeds, and showing an efficiency varying between 30 and 50 per cent. He found, however, that he got as much as 80 to 85 per cent., which was as much as could be got with a chain. He had two motors, so that the speed or power could be varied at will. One motor would propel the car along a level road, and it would run down an incline of 1 in 70 by its own momentum, which showed how little friction there was. The two motors in parallel arc would drive it up an incline of 1 in 18, and the two were used in series at starting, when the electric resistance was greatest, so that the current flowing then should be the smallest possible. With regard to the wonderfully lasting accumulators which it was said M. Julien had discovered, he would only say that if the plates lasted only six or eight months electrical cars would still be more economical than horses. Although the efficiency of the whole apparatus between the steam-engine and the car-wheels could not be taken at more than 30 to 40 per cent. (he reckoned it at 33), still it was economical to use electricity, because the accumulators only weighed $1\frac{1}{4}$ ton, and the whole of the rest of the machinery weighed half a ton, making $1\frac{3}{4}$ ton altogether, which compared very favorably with a locomotive weighing eight to ten tons. You could therefore afford to lose 60 to 70 per cent. of your motive power. He ought to say that he was much indebted to the Electric Power Storage Company

who had assisted him very materially in bringing this car to perfection. He had been working at it for four years, but it was only within the last twelve months he had arrived at a practical result. The first question was, to produce an accumulator which was light, and at the same time durable and efficient. The main advantages of this system were, first, economy, and, second, the electric car had the same appearance as those in general use, which could be easily converted, and at a small cost. The wearing parts of the mechanism were very few, and could be easily replaced. The weight of motive power was less than two tons, distributed over two small bogies of four wheels each, so that the actual weight on a given section of the rail was less than that of an ordinary car resting on four wheels only. The propelling apparatus was invisible to the passengers; the motor was boxed in completely; it was practically noiseless and free from danger. One man, not necessarily skilled, could drive the car, which could also be illumined at night by the electric current. Of course, the maintenance of the permanent way would cost less than where horses were used, and the charging stations would occupy less space than the stables.

Mr. W. R. Rowan could not agree with the two previous speakers. As far as he could gather, the contest at Antwerp was really between two combined cars, the electric car, and the steam-car which he had designed. Having been there some eight or ten weeks, he could bear testimony to the great care and trouble taken by the jury over this question. There was no doubt that the most taking car was the electric motor, but the whole question, from the first, hinged on one word—economy, and the question would turn out to be, Which was most economical, steam used directly in driving the car, or to produce electricity or compress air, which should then drive it? The electric car had all the advantages on its side as regarded appearance, and other qualities which might be desired in a motor for towns, but he thought it would be difficult for the jury to decide on the question of economy. There were four main difficulties which he thought it would take electricians some time to get over. The first was the transmission of the power. The use of a belt or chain was not generally supposed to be satisfactory, and he had his doubts about the worm-wheel. If electricians could discover a gearing which would enable them to work from a shaft running at 1,000, or even 500, revolutions a minute, those who employed steam would be very glad, because they could use it with small engines running rapidly. Such a style of engine was what naturally suggested itself for this purpose, but hitherto it could not be used on account of the difficulty in transmitting the power. The second difficulty was that of the accumulators; would they really last if knocked about daily in a tramcar? The trial at Antwerp gave very little information on that point, being short, straight, and level, so that they only had to be charged once a day. Had it been a line such as Mr. Reckenzaun mentioned, they would require charging four or five times a day. There was as yet no proof that the accumulators would stand, and if they had to be renewed even every year, the cost would be greater than that of maintaining a properly-constructed steam-engine. On a recent visit to Brussels he learned that the Company had voted 25,000 francs toward the establishment of a regular electric service on one of the lines there, in order to test this question; but he felt pretty confident that one or two years' trial would not lead to horses being superseded by electricity. The third difficulty was the charging of the accumulators. In running up steep grades, and with grooved rails, which added greatly to the friction, he believed the accumulators would have to be charged five times a day, which would be a practical difficulty. Lastly, if all these difficulties could be overcome, he doubted if they could work as cheaply as with steam direct, seeing that only 25 to 40 per cent. of the power would be utilized. The question of the power utilized, however, did not play an important part in tramway maintenance, the whole secret of mechanical tramway work would lie in having a motor powerful enough under certain circumstances, which would run very cheaply when it had little work to do, but which would work expansively, so that you could put on a second car and carry a large number of passengers when required; and

up to the present, he ventured to say, there was nothing to compete with a simple form of steam-engine, where you had only to put a little more coal on the fire to obtain the necessary power.

Mr. Scott Russell thought it a pity Captain Galton had not given some information about the *Chemins de Fer Vicinaux*, because in Ireland that sort of railways was being begun. The other day he went over one of these lines, running partly on rails and partly along the road. On the road the locomotives were made to condense their steam; on the railway they worked in the ordinary way. The paper really had not much bearing on tramways in this country, because the conditions were so very different. The rail at Antwerp was an edged rail, in England they were grooved, which gave treble the resistance; and here we had heavy inclines, and in nearly every case the engine or horses had to be detached and turned round. Abroad, steam-engines puffed through the stations without any attempt at condensation; but here the local authorities were so strict that if there were any chance of arriving at perfect condensation, they would certainly insist upon it. He agreed with a great deal of what Mr. Rowan had said with regard to using steam direct; it must be a question of economy, and however much the public liked it, you would never find a tramway company adopting electricity or compressed air unless it paid them. From published results, it appeared that the cost of working with a detached steam-engine had been brought down to 3s. 3d. a mile, and, no doubt, with a combined car, it would be still less, as there would be less dead weight; with horses it was 4s. 8d., and compressed air, taking the results at Nantes, was 5d. a mile. He was rather a believer in compressed air, but did not think either the Mekarski or Beaumont system was likely to be successful, the dead weight being too great. The loss in the compression of air was so enormous, that unless the weight were reduced to a minimum, it could not be worked economically. A much more practical plan had been proposed in America, making a very light car with a low pressure of the air, and tubes carried along the center of the track, so that the car might be charged when it stopped, by means of a coupling similar to that of the air brake. With regard to electricity, he understood from practical electricians that the theoretical loss from the engines to the accumulators, from them to the dynamo, and from that to the rail, was something like 75 per cent. as a minimum, and he was afraid, unless accumulators were made much lighter, they would be found too heavy.

Mr. Scott Moncrieff said Captain Galton had pointed out what would be admitted more and more as time went on, that certain points, such as simplicity of construction, power of moving in either direction, applicability to existing lines, &c., were of vital importance, and especially that the motor should be self-contained for the purpose of locomotion in crowded thoroughfares. This was the first paper of importance on tramway traction independent of particular interests, and embracing the subject as a whole, and it was very satisfactory that it should be based on a series of practical trials such as those made at Antwerp. At the same time, he agreed that the trials were very unsatisfactory, for the reasons which had been pointed out. The absence of inclines and curves was a fatal objection, because the best motor at Antwerp might get a prize, on the strength of which it might be introduced in a place where it would prove a complete failure. He hoped that public attention being drawn to the matter would lead to similar trials being made in this country before long. Time did not allow of his referring at length to his own work in this direction, but he might say that his first series of experiments with compressed air was the means of inducing the Legislature to permit the use of mechanical traction on urban or suburban tram lines. In 1877 power was given, subject to the approval of the Board of Trade, for mechanical power upon a practically country line at Wantage, and also an urban line in the Vale of Clyde, and his evidence with regard to the success of compressed air led the Parliamentary Committee to recommend the passing of a bill sanctioning the use of mechanical power. In common with many others, he had been waiting for the ripening of public opinion with regard to the use of mechanical power

other than steam. The pressure brought by the Board of Trade on inventors to produce something which would comply with their rules had been more or less relaxed, because no steam-engine had hitherto complied with those requirements all day long. During a certain period, or under certain conditions, the smoke and steam might be successfully suppressed, but after a certain amount of wear and tear it had been absolutely necessary for the Board of Trade requirements to be relaxed, if steam was to be applied to tramways at all. He could not speak as an expert in the matter of electricity, but its advantages, as regarded a self-contained vehicle of good appearance, were evident. Nothing surprised him more in Table No. II. than the extraordinary amount of fuel used by the compressed-air engine. It certainly bore no relation at all to the theoretical laws which bore on the conversion of energy from the boiler to the reservoir, and showed some egregious failure in the system. His own experience, as given in evidence in 1877, more nearly approached the Rowan engine, which stood at the head of the list for economy; but, as had been very properly said, if the proper motor were obtained, neither the public nor the companies would cut the matter so fine as to reject it on account of one or two lbs. of coal per train mile. He had urged before the Institute of Mechanical Engineers reasons for believing that in this particular case compressed air would be quite as economical as steam, the most obvious of these reasons being that a very large and powerful engine, embracing all modern improvements, and using a very small quantity of coal per indicated horse-power, might be employed. If such an engine only consumed 3 lbs. of coal, and there were a loss of 50 per cent. on the use of compressed air—which there ought not to be—you would get to practically the same economy as in the Rowan engine, at Antwerp, 5.42 lbs. Then the car itself, although somewhat heavier than an electric car, had still all the good qualities of a self-contained vehicle, capable of surmounting considerable inclines and going round sharp curves. He could not sit down without saying a word on the electric car described by Mr. Reckenzaun. He had long thought that the worm-wheel had been a too much abused mechanical appliance, and was not astonished to hear that the loss from friction at high velocities was much less than had been generally supposed. He should be interested to know how this worked in actual practice, but thought, from the appearance of the drawing, that it was too slight for the work it had to do.

Mr. B. Drake said that there were one or two points in connection with the Electric Power Storage Company, which he represented, which he should like to mention. They not only used accumulators at their own works, but had supplied them for trials elsewhere, and in practical working it was found that giving them the full charge, the efficiency in ampères was 75 per cent., and beyond that the loss in energy, expressed in watts, depended on the time at disposal to discharge them. When Mr. Reckenzaun tested them at the works, the power going into the accumulators necessary for each car was reduced to a minimum, and in those trials they were charged up rather quicker than they would be for lighting purposes, and there the efficiency was about seventy per cent. in watts. That experiment had been very carefully conducted, and might be depended upon. With regard to the duration, the same as had been in use for two years were now to be seen at the works; there was very little destruction of the plates, and what there was, was from mere mechanical shaking about. They were all perfectly ready, and if they were wanted to-morrow for an electric car could be put into regular work. M. Julien claimed a good deal in the way of production of local action by his special arrangement, but if one went into the causes of local action it would be seen that, if pure peroxide and pure lead were used, there was no necessity to have any local action at all. They had left batteries standing for six weeks fully charged, and at the end of the time the power was practically equal to what was put in. Whether they were discharged in one week or six weeks, the loss was hardly appreciable. The car at the works, from one cause or another, had not been used for some months, but still they were able to show it that day running without having been charged or touched for months. It had been stated

on behalf of the compressed-air car, that stationary engines more economical than any which could be used on a car were applicable; this would also hold good in accumulators with this additional advantage, that in the case of compressed air, assuming that from the moment you started you had by some mechanical arrangement to get over the gradual decrease of power from the air chambers, whereas with accumulators there was practically no loss of locomotor force, from the time of starting until the time when they had gone so low that for the sake of sparing the plates you should not discharge them any further.

Mr. M. Holroyd Smith said a good deal had already been said which it was unnecessary for him to repeat, and he only needed to clinch the argument. Mr. Rowan and Mr. Reckenzaun had made comparisons between accumulators and steam traction, the former pointing out that the difficulty with accumulators was, that you had to take the power and convert it into electric energy, and then reconvert it into mechanical energy, and that there must be a loss in the transmission. With that he perfectly agreed; on the other hand, Mr. Rowan showed that the tests at Antwerp were unfair on account of the car running on a level without curves, and that where 2 horse-power would be sufficient on a level it would have to be raised to 8 on a gradient of 1 in 25. Now, he wished to prove that, especially in the case of accumulators, that meant an excessive demand on the stored energy contained in them, and he would venture to prophesy that Mr. Reckenzaun would find that by the manner in which he was now working the accumulators they would very soon be worn out, and he did not see how a tram line could be worked economically if the motive battery in the car lasted only six months. However, he believed he had absolutely solved the question which Captain Douglas Galton had put before them, viz., that, whether electricity could be applied with certainty to the propulsion of cars. He was now doing that, day in and day out, from morning to night, at Blackpool. He used electricity direct, and, therefore, there was no loss through the accumulators. The great question with regard to using electricity direct for street purposes was how to do it with safety to the public, and until he laid down this line it had not been done, because one rail had been used for the positive and the other for the negative current, or some side rail, which would be very dangerous to the traffic, had been applied. By conveying the electricity underground, and having a means of communicating with it in the car, it was possible to utilize it without any danger, either to the public or the traffic. That point had been most carefully tested by Major Armstrong. The Major felt a little trepidation at first, and it was not until holding his hand he touched the surface of the center channel and the surface of the rails at the same time, and could feel no possible shock, that he was satisfied of the fact. The cars were running every day except Sundays; the present being the off-season, there are only three, whereas, during the season there would be ten, and of course the same plant had to be put down, and the same electrician, and standing wages had to be paid as if the whole ten were running. It was, therefore, very unfair to take the present experience as a test of what could be done, because when the whole of the cars were running it only meant a little more coal in the engine; but even now the cost was only half that of horsing. On the same line, before the electric plant was completed, horses were employed, and being just at the close of the season they were obtained from omnibus proprietors at a very low price, but still it was double the cost of electricity.

Mr. Scott Moncrieff asked what was the actual sum paid.

Mr. M. Holroyd Smith said it was rather less than 6d. per mile for the horses, but they were now doing it at half that price; when the whole ten cars were running it would cost about 2½d. per mile, including depreciation and interest on outlay. The length of the line was over two miles, the steepest gradient 1 in 40, and the sharpest curve one of 48 feet radius, curve and gradient at the same place. The point he wished to insist upon was the advantage of driving tram-cars direct instead of using accumulators. He did not wish to argue against secondary batteries for tram-car work, for he thought they would serve a very useful and important purpose, by increasing the public confidence in electricity,

and then they would be prepared to go the whole length, and use the direct current. At the same time, if you had to go up a gradient of 1 in 20 for a mile and a half, and another of 1 in 30 for two miles, as was the case in some places in Yorkshire, it required some pulling up, and unless the amount of accumulators in the car were doubled they would very soon be spent. Electricians knew that it was a very risky thing to quickly exhaust a secondary battery; you might charge them quickly if you liked, but it was a very awkward business to discharge them rapidly. The weight, therefore, would have to be increased, or their life would be shortened. Now, by his system, if the tram-car came to a hill, the man in charge only had to put on a little more resistance, and if necessary, the whole power generated at the station might be taken to send the car up the gradient. He could not only take the car, but could draw another behind it, loaded with passengers, up the steepest incline. Although they had been working through wet, wind, and snow, and the whole line had been deluged with sea water, they had been able to work with a loss from leakage of not more than the ampère of current.

Captain Douglas Galton, in reply, said he agreed with the comments which had been made on the line at Antwerp, which certainly did not represent those found in this country, but those who knew Antwerp would be aware that it was impos sible to lay down a line there with steep gradients, and even sharp curves could not be made conveniently, having regard to other traffic on the boulevards. The full report of the jury would soon be published in a very complete form, and he hoped all persons would reserve their final opinion until then.

Mr. C. J. W. Jakeman writes:—I have heard Captain Douglas Galton with considerable pleasure, as his paper differs from many on this subject, in that it contains reliable figures and data, gathered from specially conducted public trials. I may say that my firm (Merryweather & Sons) was invited to take part in the trials, but after examining the road, decided that it would be no fair test of the practical working of the engines on ordinary town tramways, with grooved rails, and consequently did not enter the contest. From the figures given in the paper, it appears that Rowan's combined car was most economical in working, and that very little water (three quarters of a gallon per mile only) was required to make up loss; this was probably due to the fact that on a combined car, considerable roof-space is available for the condenser, and not to its construction; as in our experiments with plate condensers we have always found them inferior to tubulous ones, which we have finally adopted. With reference to this point, I may say that with our detached engines working on the North London tramways, with inclines of 1 to 20, only from 10 to 20 gallons of fresh water are required after a run of 11 miles.

The figures relating to the compressed air car are really astonishing, and, unless the principle of working air at high pressure is altogether wrong, as it possibly is, there must have been some very bad management in the working. The coke alone (3 lbs. per mile), used to warm the air, would have been more than half the quantity of fuel required to drive by steam direct.

The average heat value of well purified coal-gas at constant volume has been recently determined by M. Witz—*Ann. de Chim. et de Phys.*—as about 5,200 calories per cubic meter at 0 deg., and 760 mm. when the water formed is fully condensed. This value, got from a great variety of experiments with gas from different works, appears to make the generally accepted figure of 6,000 calories about 15 per cent. too high, and the calculation of gas motors is here concerned. The heat value of the gas from one and the same works varied in the course of a year from 4,719 to 5,425 calories, which was more than the variation between different works. The influence of temperature and external pressure was not perceptible. The operations for purifying gas diminish the heat effect sometimes as much as 5 per cent. The gas of the last hour of distillation is, *Nature* says —contrary to the usual view—less rich than that of the first hour. Dilution with oxygen lessens the heat value; but in dilution with air, curiously, no such effect is observed; the heat of combustion was the same with six or with ten volumes of air.

CORROSIVE EFFECTS OF STEEL ON IRON IN SALT WATER.

By J. FARQUHARSON, Esq.

Read before the Institution of Naval Architects.

At the meeting of this Institution last year, attention was drawn to this subject, and some particulars furnished of actual cases of rapid corrosion. The facts then stated were rather suggestive than conclusive that its origin was the steel combination, as there are other known causes of equally rapid corrosion where no steel is present. Large iron forgings, besides being liable to external influences, contain within themselves elements of decay as rapid as any then noticed. Such forgings are made up of numerous smaller ones, and after being welded up into one whole, they contain more or less magnetic oxide, which is as destructive as a like quantity of copper would be if placed in its stead; the well-known fissures, or deep seams which appear more or less in all rolled or forged iron when corroded by salt water, are wholly due to this cause; these fissures bear, in direction, a certain relation to one another, by which they are readily known when the actual case is before us, but not otherwise. Although the Admiralty practice does not involve combinations of iron and steel to any great extent, the question raised last year was considered of sufficient importance to test, by actual experiment, the results of which I am now permitted to bring before you. Before doing so, in order that the basis of the experiment may be clearly understood, it may be well to notice, briefly, another experiment, made two or three years ago for the purpose of testing the effects of surface oxide, or scale, on rolled mild steel. The two points which that experiment was designed to ascertain were, first, the amount of injury by pitting which the scale might cause in a given time when portions of the surface are unprotected by such scale; secondly, whether such scale action is likely to be permanent. The result went to show that there is practically no diminution at the end of six months' immersion in salt water; steel plates completely covered by scale in combination with a similar steel plate without scale, in some cases did not lose a single grain in weight. Second, the loss of weight, or work done by steel oxide, was found to be rather more than from a plate of copper of the same size. The experiment now about to be described was therefore undertaken with a full knowledge of these results, which account for much of the confusion and misapprehension which have arisen in cases where scale was neglected, and which show that in any case intended to test relative corrosion of metals, surface scale, or oxide must not be neglected; that care must be taken that the materials used are iron and steel, and nothing else, and that the surfaces be large enough to give a good average result. In the present case, plates of iron and steel of equal size, with an aggregate surface of 48 superficial feet, were used. After having the scale completely removed by dilute hydrochloric acid, they were singly weighed, marked, and placed in a grooved wooden frame, parallel and one inch apart, iron and steel alternately. The first, third, and fifth pairs were electrically combined by straps of iron at the tops, the second, fourth, and sixth pairs being left unconnected, and therefore each plate of which was only subject to ordinary corrosion, as if no other metal existed. The whole series, so arranged, were placed in Portsmouth Harbor, and left undisturbed for six months, when they were taken up and again weighed. The loss of each plate was found to be as under:

		Oz.	Gr'ns.
Steel } combined		0	427
Iron } combined		7	417
Steel		3	340
Iron		3	327
Steel } combined		0	297
Iron } combined		7	77
Steel		4	0
Iron		3	190
Steel } combined		2	337
Iron } combined		6	0
Steel		4	157
Iron		4	57

From the above it will be seen that the three iron plates combined with steel lost 21 ounces 57 grains; that the three similar iron plates not combined lost only 11 ounces 137 grains. The plates were identical in size, and all cut from the same sheet, the effect of combination with steel being to nearly double the loss of weight. The proof that the great excess of loss was not due to anything in the plates themselves will be clearly seen by comparing the combined and uncombined steel plates thus: The three combined with iron lost only 4 ounces 187 grains; the three uncombined lost 12 ounces 60 grains, or nearly three times as much as those protected electrically by the iron. These two facts taken together, viz., iron combined with steel invariably lost more, and that steel so combined lost less, prove to a demonstration that electrical action existed. The difference in such loss of weight is a measure of the amount of such action from which it would be easy to draw wrong conclusions. One thing may be inferred, viz., that in this particular case about two-thirds of the electrical energy of the combination was given up in reducing the metal, and the other one-third in the intervening liquid. Taking the distance apart into consideration, it will be seen that the energy was considerable. The laws of electro-chemical action are brief, simple, and invariable, but the results are so modified by conditions which interfere in practice that a clear appreciation of them is necessary in in each particular case. It would not be safe to infer that if these plates had only been one-fourth the distance apart, the loss would have followed the well-known law, because, in such a case it is probable that the action would soon have been arrested by the formation of rust between the plates; on the other hand, if the iron plates had been protected, except a patch in the middle, by waterproof material, all other things remaining the same, it is quite certain that the result would have been serious injury to the plate by pitting of the exposed part, on which a very large portion of the energy in such a case would have been concentrated. Again, had the plates been placed edge to edge and contact maintained, the iron would certainly have suffered much on the edge next the steel. Before leaving this part of the subject, a word of explanation with reference to a difference which may be noticed in the relative loss of the third pair of combined plates is necessary, in which case the steel lost more and the iron less than in other similar pairs. The probable cause of this is that the connection at the top was less perfect than it should have been, and that, in consequence, rust formed between the connecting strap and the plate, which after a time arrested the electrical energy and reduced it to a case of simple corrosion. With this exception, which is not a large one, the results are fairly uniform, when judged in the light of local influences which may exist in the individual plates.

The main object of this experiment was to test the effects of combinations of iron and steel, and the lesson taught is to either avoid altogether such combinations, or to take care to so modify the conditions as to minimize the injury to the iron; but you will observe that the arrangements in this experiment are such that the results may throw some light on a still more important question, viz., the relative endurance of iron and steel when freed from injurious combinations. The already extensive and still growing use of steel makes this a matter of very great importance. Assuming that the unconnected plates of steel and iron represent the normal loss of each under ordinary and equal conditions, they approximate so closely that the endurance may be considered as practically the same, and this result agrees with that obtained from other and more extensive tests previously made for the Admiralty, and which formed the basis and justification of the use of steel instead of iron in naval construction. In the present experiment the unconnected plates had an aggregate surface of 12 superficial feet each metal. The total loss of weight was—iron, 11 ounces 137 grains; steel, 12 ounces 60 grains; difference in favor of iron on the whole surface, 360 grains weight, or 30 grains per foot superficial, which is inconsiderable. A careful examination of the steel plates in this and other cases, after immersion, convinces me that these results (satisfactory as they may be when taken in conjunction with the other advantages of steel) are neither as good as they might be, or as

they would be, if the importance of uniformity were recognized, and the ingredients thoroughly mixed, as they should be in the process of manufacture. In almost every plate there are evidences of local action between one portion and another—a sure indication that the manganese is not evenly diffused throughout. The plates used in this experiment are here, and we advised all interested to examine them and judge for themselves. In a former case, in which the surfaces before immersion had been finished bright by fine filing, the marks could be seen, after six months, on some parts, whilst other parts of the same plate were well corroded. In the present case the plates were not so prepared, but there are equally clear evidences of the facts observable. I commend this matter to the attention of steel-makers and steel-users, who are both interested. It has been said that manganese is difficult of diffusion, but if its importance as affecting the durability of the steel is recognized, means would soon be found to improve it. Knowing the facts stated above, on a recent visit to steel works I took careful note of the practice, which was as follows: The furnace was tapped, and as soon as the molten steel began to run into the ladle, two men, each with a shovel, began to throw the ferro-manganese into the ladle, and this they continued to do until the steel was all in; no steps whatever were taken to mix by stirring or agitation. In such a process the wonder is that the results are no worse than they are found to be. When the ingredients are thoroughly mixed, there is good reason to believe that the endurance of steel, as regards corrosion, will not only be equal to the best iron, but far superior, as it ought to be; and the time may not be far distant when consumers may find a ready means for detecting inequalities which will help to secure attention to this important matter.

DISCUSSION.

Mr. Mesham. My lord, I thought, perhaps, it would interest the meeting—I will not detain you long—to show you samples of iron treated by a process whereby this magnetic oxide is produced by artificial means, where a portion of the plate has stood two years in continuous submersion in salt water. I have only to add that I hope the time will soon come when this process will be largely used for the purpose of protecting steel and iron ship plates. I have this piece of iron, which is at anyone's disposal, and I think you will all admit there is not a particle of rust upon it of any kind.

Capt. Watt (of Liverpool). My lord, I have a little experience which points to exactly the reverse of that. The conditions are not exactly the same, because the vessel that I sail in was employed in salt water. The screw was worn away on the leading edge, and was patched with mild steel. We found that the mild steel was eaten away by the cast iron. In that case the steel suffered far more than iron.

Mr. B. Martell. My lord, I have only a few words to add, and these are in corroboration of what Captain Watt has said. It only shows the mystery that underlies all these things. I think we are very much indebted to the Admiralty for placing the results of their experiments before us in this way. The question of steel for ship-building purposes is now a question of very great importance, not only to the Admiralty, but to the mercantile marine also, and they are not only looking to the quality of it, and to these mysterious symptoms which are said sometimes to occur, but also to its durability. That is a matter that is occupying the very serious attention of many shipowners at the present time. Any information that can be brought before them in the way of real practical experience of this kind is a matter of great importance. As I said before, we are very much indebted to the Admiralty for placing before the public any experiments they make in this way. I would remark, that in the case of a ship built some little time ago (a steel vessel that was riveted with iron rivets), I had an opportunity of seeing her after she had been running twelve months, and I then found that the steel plates in the immediate proximity of the iron rivets had deteriorated very considerably beyond what the iron rivets had; the rivet points protruding some distance beyond the steel plates, while the steel around the iron rivets had deteriorated very considerably. I am sorry that the builder of that ship—Mr. Raylton Dixon—is not here; he was here

this morning to make some remarks upon that point. That appeared to me to show that this deterioration was not due to mere corrosion, but was probably due to galvanic action, from the rapidity with which it occurred, and from its being in so many places immediately round the rivet points. Although we see the plates placed before us here, and can place implicit reliance on the experiments made by Mr. Farquharson, showing the results to be almost invariably that the iron has suffered most from this action, yet, in the case I have mentioned, the steel had deteriorated more than the iron.

Mr. Barnes. Mr. Martell, would you kindly inform the meeting what the nature of the iron rivets was? Was it Bowling, Lowmoor, or ordinary Staffordshire?

Mr. Martell. I wish, as I said before, Mr. Dixon was here, but I have reason to believe that it was Continental iron—I think so—because I know at that time he was importing a large quantity of iron rivets from the Continent.

Mr. Barnes. That is very important.

Mr. W. Denny. I think the paper Mr. Farquharson has brought before us is one for which we have every reason to be thankful, but it is not one that can be easily discussed off hand, because, as Mr. Farquharson has pointed out, there are many matters in it, especially these extraordinary-looking lines in the specimens (pointing), which are rather suggestive than capable of solution at this meeting, or perhaps for several meetings to come. There are many points about the corrosion of steel, and also the corrosion of iron by steel, that are really worthy of serious consideration. Mr. Martell has given his experience where iron rivets corroded steel plates. My firm has had an opposite experience. We built for the Peninsular and Oriental Company their first steel steamer, the *Ravenna*, and I drew the attention of the Institution last year to the fact that, while the whole of the hull of the *Ravenna* was steel and the rivets were steel, the only portions that were iron were the forgings and certain covering plates in the rudder—the plates covering the distances between the pintles, the actual rudder plates being steel. What we found in the case of the *Ravenna* was this, that there was a large corrosion (which you can see for yourselves, because here is a cast of it) at the upper rudder band of the *Ravenna's* rudder-post. That corrosion goes in for nearly $\frac{3}{16}$ of an inch, pitting into the iron rudder-post. There was a further corrosion, although not so serious, in some portions of the iron rudder, and there was some little corrosion also in the iron covering plates of the rudder. There was no corrosion whatever in the steel plates of the rudder, showing that whatever effect was produced affected simply the iron and nothing else. While I do not think we can gather anything very conclusive from this paper, or from the experience Mr. Martell, my own firm, and the Peninsular and Oriental Company have had, with regard to the cause of the actual effects produced, there is one conclusion we can draw from them, that it is a very unsafe thing to put any other metal with steel under water. On that account my firm have adopted the practice, in all our steel steamers, of using only steel rivets and steel forgings. Since we have done that we have not, so far, observed any of these defects. There is another point of importance on the subject of corrosion: I had occasion to examine the bottom of a steel steamer lately, and, among other parts, the inlet where the water was taken in for the condenser. The inlet was covered by a large brass plate with holes in it. When that brass plate was removed we were perfectly astonished at the corrosion which had gone on, and which had eaten down into the steel to the extent of $\frac{3}{16}$, and I examined the other inlets in the same ship, and found similar corrosion. I would, therefore, call attention to the serious danger not only of bringing together iron and steel in a steel ship, but also of placing brass upon the bottom of a steel ship.

A member. May I ask Mr. Denny whether the rudder bearings were bushed with metal in the case of the *Ravenna?*

Mr. Denny. I think it was lignum-vitæ.

Mr. W. W. Rundell. My lord, I should like to ask a previous speaker a question with regard to the example showing corrosion of the plates instead of the rivets; I should like to ask in what sea the vessel sailed, in what harbor she was lying, and under what circumstances these apparently curious occurrences happened. I

am not surprised to hear that two pieces of iron may mutually act on one another electrically, because I remember once having a battery composed entirely of plates of iron cut from the same sheet, and therefore of the same chemical composition, but yet these plates of iron formed a most powerful battery, and for this reason, that the plates in each pair were placed in opposite electrical conditions by being immersed in different fluids with a thin diaphragm between; one was excited by nitric acid, and the other was excited by an alkaline solution. They were thus placed in very different conditions, although they were plates from the same material. If we knew the character of the water in which the vessel in question had been lying it would throw some light on the particular circumstances of the case. I should like to ask if it is known in what sea or harbor that vessel had been lying. The mysterious symptoms would probably disappear if the facts were clearly stated.

Mr. Denny. With reference to which ship—the question of the corrosion of the iron by the steel?

Mr. Rundell. I referred to the case mentioned by Mr. Martell—the vessel built by Mr. Dixon.

Mr. Martell. The liquid in which these vessels were was salt water.

Mr. Manuel. May I ask Mr. Martell if he knows from his own experience the action of the iron rivets used in riveting on the mild steel shell plates of the steamer *Ethel*, built in 1878? It so happened that it was a new departure to construct ships of mild steel in the district where I had the honor to serve as engineer surveyor to Lloyd's Registry, and the rivet boys in heating the steel rivets destroyed them. It was then thought under these circumstances, until we found out the cause of the steel rivets becoming weak and unfit, to substitute iron rivets instead of the steel rivets, and as this vessel is periodically surveyed by Lloyd's Registry, Mr. Martell may be able to tell us now, from the time that steamer has been running in salt water, whether the iron rivets are affected by the steel plates, or the steel plates by the iron rivets. That was in 1878. With regard to Mr. Denny's mention of a steamer belonging to the Peninsular and Oriental Company which had steel plates in the rudder, the frame of which was formed of iron, it will be interesting to know, when that steamer returns, the result of putting in iron rivets instead of steel to keep these plates close again, which Mr. Denny says were the cause of the corrosion of the iron. I shall be able to give at some future time some more information with regard to this. The steel rivets which had become loose in the rudder were replaced by iron rivets. I am not quite of the same opinion, and I do not think that there is any such action as Mr. Denny seems to expect. It is very difficult and a very critical thing in constructing a steel steamer to get everything of steel throughout. I do not think if the steel and the iron are faithfully put together there will be very much to fear from the action of the metals. With regard to the corrosion of steel on screw propellers mentioned by Captain Watt, I have had a little experience, and I have not found that even metal such as brass or Muntz metal when put on steel blades has such a bad action as has been stated. For instance, the steamer *Lombardy* has Vickers' steel blades. I think she is at Messrs. Caird & Co.'s yard at present, but she was built by Messrs. Denny & Co. These steel blades corroded so rapidly—in fifteen months or less—that it was found necessary to do something to protect them, and we sheathed them at ends of blades with brass plates with good results, and these blades have now been running for five or six years, and are intact still. Since that I have used this mild steel of twenty-six tons to the square inch on behalf of the Peninsular and Oriental Company for the construction of some propeller blades on account of its superior strength, but again they were subject to this same corrosive action at blade points by salt water. We found that while we got steel blades to stand as regards strength, they corroded very rapidly in one voyage of three months, and to protect them I covered them with brass sheathing, and we have found this to be a marked success; the blades have not deteriorated further, although sheathing was put on in a hurried manner in dry dock, as close as it could be done by workmen. It stands well, and I believe it will be successful, and that we shall be able to keep on the steel blades without

having to renew them by such an expensive method as new ones every eighteen months or two years, which has been the case with some of the steamers in the Liverpool mail service. I think Messrs. Vickers fully agree with what I say. They have seen the blades after they had run two voyages, and are quite pleased with the result of putting brass on to steel to prevent corrosion of steel by salt water.

Mr. W. H. White. My lord, I will say a word in reply to the observations of Mr. Denny. Mr. Denny said, as I understood him, that it was undesirable to have brass under water fittings in ships.

Mr. Denny. Yes.

Mr. W. H. White. In the Admiralty service we always have brass under water fittings, and the bottom plates do not suffer in the way Mr. Denny has experienced, because we fit protectors of some other metal on them, which will suffer from the action of the brass, and leave the skin of the ship intact. Of course, the principle of protection may be carried further. I will only add that I think the value of this paper will appear on closer reading, but we have here really a large laboratory experiment rather than an example of common practice. We have not the surface protected by anticorrosive paint at all. It is quite possible the results attained here with bared surfaces are exaggerations, as I think Mr. Farquharson will tell us, as compared with anything we could expect to get in the way of wear in actual practice on the bottoms of ships protected by paints. In the experiments the plates had this bare surface, and were immersed in sea water and fully exposed.

Mr. Farquharson. My lord, first let me refer to a remark made by Mr. White before I forget it. It is quite true that these experiments were made expressly to put the two metals on an equal footing, and under the worst conditions possible. The object of it was to ascertain what effect steel had on iron, or iron on steel; therefore, it was necessary, as it is in all experiments, to carefully provide against any extraneous circumstances that would influence the result. Now, with regard to Capt. Watt's observations, I need only say that those circumstances were different, and cannot be judged of from anything you see here or what I have said. Mr. Martell has made some interesting remarks with regard to what occurred around iron rivets. That, again, is a case that I should have very much liked to have seen, because there are many circumstances to be taken into consideration that might modify the results. These results follow, and will follow, all similar arrangements placed as that was, but there are many circumstances, I need hardly tell you, that influence it very much. The formation of an oxide on one metal less exposed to the wash of the water than the other, at once changes the direction of the current, and would, therefore, throw the action on the opposite metal. A remark has been made with regard to an iron battery, which leads me to say that we expect, in all wrought iron, conditions which would produce almost anything that you like, provided that you only let us arrange it in the order that will produce that. I think, sir, I need hardly trouble you with any further remarks on the matter, as I think the facts are not challenged.

The President. I am sure you will allow me to convey, not only our formal thanks to Mr. Farquharson, but also will allow me to point out to you that we are under a special obligation to Mr. Farquharson, because he is not a member of our institution, and he has conducted the experiments with the greatest care. I am sure you will see from his paper, and gather from his remarks likewise, these valuable experiments, which, I venture to think are of national importance to all users of steel. Therefore you will allow me to convey a special vote of thanks to Mr. Farquharson for his paper, and for the ability with which he has conducted for the Admiralty, and the public in general, these most valuable experiments.

THE mortar used for the external brick facing of the Forth Bridge piers below water, says *Engineering*, consists of one part of Portland cement and one part of sand lightly ground together in a mill with salt water. The average tensile strength of samples taken from the mill is 365 lbs. per square inch at one week, and 510 lbs. at five weeks after mixing.

EXPERIMENTS TO DETERMINE FRICTIONAL RESISTANCES OF RAILWAY TRAINS.

By C. H. HUDSON.

Journal of the Association of Engineering Societies.

This paper is descriptive of a series of experiments made by the writer a few years ago for the purpose of furnishing some evidence in a pending lawsuit between two railroad companies. The questions studied were the resistances of the trains and the powers of the engines. The subject was not a new one, the ground having been gone over many times; but possibly the treatment of it in this case may vary somewhat from other methods, as shown by published records. The means adopted were such as we deemed best to furnish us with the information needed at the time, and, though somewhat crude, the end was attained. The experiments themselves were not as extended or varied as they probably would have been, had any other use been expected of them. Those relating to the resistance consisted in ascertaining in what time and distance a train of a given weight, and moving at a known speed upon given grades, would stop, and from the data obtained we have made our calculations.

The time was measured by watches usually used for timing horses, and to be sure that no mistakes were made they were in the hands of several people, so that in no case did less than two people observe the times taken. Distances were accurately measured in the usual way. No special pains were taken to select engines or cars in good order. The latter were taken from the side track as they chanced to stand, loaded with coal, and accurate weights taken. The engines, in the same way, were taken as they came, and were at liberty; but they were in good condition, and of the ordinary "American" type, of dimensions hereafter shown. The cars were ordinary flat-bottom coal-cars, or gondolas, with thirty-three-inch wheels, three and one-half by six and one-half journal, brass bearings, and lubricated with black oil of rather inferior quality.

The dimensions and weights of the engines as used, and of the cars, with their loads, are as follows:

Engine No. 48.—Cylinders, 17 in. diameter, 24 in. stroke; driving wheels, 62 in. diameter, four coupled, with four-wheel truck, "American" style; tender on two four-wheeled trucks. Weight of engine on trucks, 27,450 lbs. (as it came in from the road, with about three-fourths tank water and one-half tank of coal); weight on driving wheels, 46,300 lbs.; total weight, 73,750 lbs.; tender three-fourths full of water and one-half full of coal, 40,900 lbs.

Engine No. 47.—Same as 48.

Position in Train and Weight of Cars Used in Experiments, Numbering from South End.

Position.		Number.	Gross Weight.
1	.....	2,057	47,700
2	.....	2,315	47,900
3	.....	167	47,100
4	.....	2,189	50,700
5	.....	987	48,200
6	.....	2,061	48,200
7	.....	2,015	49,700
8	.....	405	47,700
9	.....	2,165	48,000
10	.....	203	44,900
11	.....	285	48,600
12	.....	977	47,400
13	.....	855	48,900
14	.....	2,103	48,800
15	.....	205	53,400
16	.....	2,049	43,500
17	.....	523	48,200
18	.....	2,073	47,800
19	.....	493	47,700
20	.....	383	47,100
21	.....	907	48,000
22	.....	677	44,000
23	.....	2,171	49,100
24	.....	2,287	48,000
25	.....	325	48,500
25	.....	495	49,000
27	.....	311	51,200
28	.....	2,295	51,000
29	.....	351	49,400
30	.....	2,025	47,600

Engine No. 63.—Cylinders, 17 in. diameter, 24 in. stroke; driving wheels, 58 in. diameter, four coupled, with four-wheeled truck; tender on two trucks, four wheels each; weight, engine truck, light, 23,740 lbs.; 3 gauges water, 26,557 lbs.; weight on drivers, light, 40,960 lbs.; 3 gauges water, 46,563 lbs.; total weight, light, 64,700 lbs.; 3 gauges water, 73,120 lbs.; tender weight, light, 23,100 lbs.; full of water, 42,200 lbs.; full of water and $4\frac{1}{2}$ tons coal 51,200 lbs.

Experiment 1.

Engine 48, weight	73,750 lbs.
Tender, "	40,900 "
15 cars (1 to 15 inclusive), weight	727,200 "
Total	841,850 lbs.
Length of train, including engine	533 ft.

Train got under motion on level, and steam shut off at foot of 52.8 up-grade, on tangent.

Speed measured by time taken to pass a base line 400 feet long:

Seconds passing base	23.35
Speed per second	17.17 ft.
" minute	1,028 "
" hour	11.70 mls.
Train ran by momentum	538 ft.
Time used (estimated)	63 sec.

Very light wind on right side, nearly at right angles to train.

Unfortunately, in the first six experiments no note was taken of actual time used in the stop, and we have to estimate it. We do this on the theory that the retarding force is constant (probably not exactly true in this case).

We use the formula $T=\frac{2S}{V}$, in which T=time; S=space, and V=velocity at time steam is shut off. This gives $T=\frac{1076}{17.17}=63$. The estimated times mentioned hereafter are calculated in this way.

Now, to estimate the resistance, which is the retarding force, we will again assume that it is constant. This would be true if the train was all on the 52.8 grade; but in fact it commences to enter that grade just when steam is shut off, and when the train is on a level. We take V=velocity=17.17; S=space run =538; T=time (estimated)=63. As our three items do not exactly correspond, we shall not get an exact result, but will take the three formulas for the momentum at end of first second (working from the other end, and supposing that our body moves from a stand with increased velocity till it reaches that at which, in our experiments, we start):

$$\text{First F., momentum}=\frac{V^2}{2S}=.27399$$

$$\text{Second F.,}\quad =\frac{2S}{T^2}=.27110$$

$$\text{Third F.,}\quad =\frac{V}{T}=.27270$$

$$.81779$$

$$\text{Average, } .27259$$

The momentum of a body in a uniformly accelerated motion at the end of the first second varies as the velocity, and the velocity varies as the power causing the velocity or motion. In case of a body affected by gravity we have, the power (weight of body)=1, and momentum 32.

In the case of our experiments we have the momentum .27259. Upon the basis of above relations we can take the following proportions: $1:32::x:.27259$, x being the measure of amount of force causing the acceleration (or retarding), which gives=.27259.

Reduce, and we have $x=.008519$. Now, for one ton we have $.008519\times 2{,}000=17.038$, which gives the resistance per ton causing the train to stop, providing the force acted uniformly.

As a matter of fact, the train all stood on a level save the engine itself; the tender was on the level.

The movement was 538 feet, and gravity affected the engine during the entire movement, and the tender all but its length, and so on.

On next page is a table of the weights of engines, tenders and cars, in tons, with the distance they moved on the 52.8 grade, where gravity would retard the movement at 20 lbs. per ton.

Column 1 shows engine and cars as in train. Column 2 shows weight in tons. Column 3 shows distance moved on 52.8 grades. Column 4 shows weight multiplied by distance.

1.	2.	3.	4.
Engine	36.9	538 feet.	19,852.2
Tender	20.4	518 "	10,567.2
1	23.9	486 "	11,615.4
2	23.9	454 "	10,850.6
3	23.6	422 "	9,959.2
4	25.3	390 "	9,867.0
5	24.1	358 "	8,627.8
6	24.1	326 "	7,856.6
7	24.8	294 "	7,291.2
8	23.9	262 "	6,261.8
9	24.0	230 "	5,520 0
10	22.5	198 "	4,455.0
11	24.3	166 "	4,033.8
12	23.7	134 "	3,175.8
13	24.4	102 "	2,488.8
14	24.4	70 "	1,708.0
15	26.7	38 "	1,014.6
	420.9		125,145.0

125,145×20 lbs. (due gravity)=2,502,-900.

Now, our estimated resistance to the ton, as given (p. 256), was 17.038 lbs. per ton.

17.038×420.9 (weight of train) ×538 feet (distance moved)	3,858,144
From which deduct am't due gravity.	2,502,900
We have	1,356,244

Divide by 538 (distance moved) we have 2,519.04; again divide by 420.9 (total tons) we have 5.984 lbs. for the resistance due other causes than gravity.

Experiment 2.—Same train, place and conditions.

Time passing base	28.9 sec.
Speed per second	13.83 ft.
" minute	830 ft.
" hour	9.43 m.
Distance run	403 ft.
Time (estimated)	58.3

As before, V=13.83, S=403, $T=\frac{2S}{V}=$ =58.3 (estimated).

Then,

$$F=\frac{V^2}{2S}=.2373$$

$$F=\frac{2S}{T^2}=.2371$$

$$F=\frac{V}{T}=.2372$$

Average, .2372

As before, 1 : 32 : : x : .2372, and x= .0074125, or per ton .0074125×2,000= 14.825.

	Tons.	Distance on 52.8 grade. Feet.	Tons × Distance.
Engine	36 9	403	14,870.7
Tender	20.4	383	7,813.2
1	23.9	351	8,388.9
2	23.9	319	7,624.1
3	23.6	287	6,773.2
4	25.3	255	6,451.5
5	24.1	223	5,374.3
6	24.1	191	4,603.1
7	24.8	159	3,943.2
8	23.9	127	3,035.3
9	24.0	95	2,280.0
10	22.5	63	1,417.5
11	24.3	31	753.3
12	23.7	0	.....
13	24.4	0	.....
14	24.4	0	.....
15	26.7	0	.....
	420 9		73,328.3

73,328 3×20 (effect of gravity on 52.8 grade)	1,466,566
420.9 (tons) ×403 (distance) ×14.825	2,514,656
Subtract amount due gravity	1,466,566
We have	1.048,090
Divide by 403 (distance)	2,600.72
Divide again by 420.9 (tons)	6.45

equals resistance from all other sources besides gravity.

Experiment 3.—Same train, place and conditions:

Time passing base	40 sec.
Speed per second	10 ft.
Speed per minute	600 "
Speed per hour	6.81 m.
Distance run	234 ft.
Time used (estimated)	47 sec.

As before,

$$V=10 \quad T=\frac{2S}{V}=47 \text{ sec.}$$

And again:

$$F=\frac{V^2}{2S}=.21367$$

$$F=\frac{2S}{T^2}=.21186$$

$$F=\frac{V}{T}=.21256$$

Average, .21269

As before, 1 : 32 :: x : .21269. x = .0066465, or, per ton = .0066465 × 2000 = 13.39 lbs.

	Tons.	Distance on grade Feet	Tons × distance.
Engine	36.9	234	8 634.6
Tender	20.4	204	4,161.6
1	23 9	172	4,110.8
2	23.9	140	3,346.0
3	23.6	108	2,548.8
4	25.3	76	1,922 8
5	24.1	44	1,060 4
6	24.1	12	289.2
7	(as before)		
8			
9			
10			
11			
12			
13			
14			
15			
	420.9		26,074.2

26.074.2 × 20 (effect of gravity).... 521.484
420.9 (tons) × 13.39 (estimated momentum) × 234 (distance). 1,318,789

Due other resistance than gravity.. 797,305
Divide by 234 (distance)..... 3,407.3
Divide again by 420.9 (tons)..... 8.09
equals resistance per ton other than gravity.

Experiment 4.—Same train, place and conditions:

Time passing base	35.4 sec.
Speed per second	11.3 ft.
Speed per minute	678 ft.
Speed per hour	7.70 m.
Distance run	316 ft.
Time used (estimated)	56 sec.

As before,

V = 11.3 S = 316 feet T = 56 seconds.

$$F = \frac{V^2}{2S} = .20204$$

$$F = \frac{2S}{T^2} = .20153$$

$$F = \frac{V}{T} = .20178$$

.60535
Average, .20179

Then, 1 : 32 :: x : .20179. x = .006306, or, per ton, .006306 × 2000 = 12.61 lbs.

	Tons.	Distance on grade. Feet.	Tons × distance.
Engine	36.9	316	8,500.4
Tender	20.4	296	6,038.4
1	23.9	264	6,309 6
2	23.9	232	5.544.8
3	23.6	200	4,720.0
4	25.3	168	4,250.4
5	24.1	136	3,277.6
6	24.1	104	2,506.4
7	24.8	72	1,785.6
8	23.9	40	956 0
9	24.1	8	192.8
10	(as before)		
11			
12			
13			
4			
15			
	420.9		44,082.0

44,082 × 20 (effect of gravity)..... 881,640
420.9 (tons) × 12.61 (estimated resistance) × 316 (distance) 1,677,185
Resistance other than gravity..... 795,545
Divide by 316 (distance)..... 2,517.55
Divide by 420.9 tons) ..... 5.95 lbs.
equals resistance other than gravity.

Experiment 5.—Same place, time and conditions:

Time passing base	25.6 sec.
Speed per second	15.62 ft.
Speed per minute	937 5 ft.
Speed per hour	10.65 m.
Distance run	495 ft.
Time (estimated)	63 sec.

Then,

V = 15.62 S = 495 T = 63.

$$F = \frac{V^2}{2S} = .2464$$

$$F = \frac{2S}{T^2} = .2488$$

$$F = \frac{V}{T} = .2479$$

.7431
Average, .2477

Then, 1 : 32 :: x : .2477. x = .00771 ; .00771 × 2000 = resistance per ton = 15.42 lbs.

	Tons.	Distance on grade Feet.	Tons × distance.
Engine	36.9	495	18,265.5
Tender	20 4	475	9,690.0
1	23.9	443	10,587.7
2	23.9	411	9,822.9
3	23.6	379	8,944.4
4	25 3	337	8,526.1
5	24.1	305	7,350.5
6	24.1	273	6,579.3
7	24 8	241	5,976.8
8	23.9	209	4,995.1
9	24.0	177	4,248.0
10	22.5	145	3,262.5
11	24.3	113	2,745.9
12	23.7	81	1,919.7
13	24.4	49	1,195.6
14	24 4	17	414.8
15	26.7	0	.....
	420.9		104,524.8

104,424.8 × 20	2,090,496
420.9 (tons) × 495 ft. × 15.42	3,212,687
Due other than gravity	1,122,191
Divide by 495	22,669
Divide again by 420.9	5.38 lbs

resistance per ton other than gravity.

Experiment 6.—Same train and time. Train entirely upon a 52.8 grade, but engine at beginning of 2 degree curve to right when shut off, train on straight line.

Time passing base (300 ft.)	18.10 sec.
Speed per second	16.57 ft.
Speed per minute	994 ft.
Speed per hour	11.33 m.
Distance run	321 ft.
Time (estimated)	39 sec.

$V=16.57 \quad S=321 \quad T=39$

$$F=\frac{V^2}{2S}=.42767$$

$$F=\frac{2S}{T^2}=.42208$$

$$F=\frac{V}{T}=.42487$$

1.27462

Average, .42484

Then, $1:32::x:.42484$. $x=.013277$; per ton $=.013277\times2000=26.554$ lbs. As whole train was on 52.8 grade, the resistance due gravity $=20$ lbs.; resistance other than due gravity, 6.554 lbs.

Experiment 8.—The trial was made upon level and straight track. Track was in fair condition, iron rail put up with sand (as was the track where all these experiments have been made). The joints were down somewhat, and the whole bed was much more elastic than if put up with coarse gravel or stone. Engine 47. Cars 16 to 30, inclusive.

Base line	300 ft.
Time passing base	17 sec.
Speed per second	17.65 ft.
Speed per minute	1,059 ft.
Speed per hour	12.08 m.
Distance run	1,500 ft.

When engine struck down grade, train was then running at speed, per second, 8.10 ft.; per minute, 4.86 ft.; per hour, 5.52 miles. Light wind on right-hand side and a little in rear. Train going north.

Now, to estimate the time:

T, the train is in passing the 1,500 ft.

8.1 T = the space passed due the velocity at end; and, $1{,}500-8.1\,T$ = the space due the retarded velocity.

Then,

$$T=2S=\frac{2(1500-8.1T)}{9.55}$$

Reduce, and $T=116.5$.

We before had

$$S=1{,}500-8.1T=1112.7$$

$$V=9.55.$$

Then,

$$F=\frac{V^2}{2S}=.08196$$

$$P=\frac{2S}{T^2}=.08368$$

$$F=\frac{V}{T}=.08368$$

Average, .08311

Then, $1:32::x:.08311$. $x=.0025973$; $.0025973\times2000=5.194$ lbs. = resistance per ton.

Experiment 9.—Same time, place and conditions:

Time passing base (300)	26.1 sec.
Speed per second	11.48 ft.
Speed per minute	689 ft.
Speed per hour	7.82 m.
Run	1,272 ft.
Time (estimated) going north	221 sec.

$V=11.48 \quad S=1{,}272$ ft. $T=$(est.)221 sec.

$$F=\frac{V^2}{2S}=.0518$$

$$F=\frac{2S}{T_,}=.0521$$

$$F=\frac{V}{T}=.0519$$

.1558

Average, .05193

Then, 1 : 32 :: x : .05193. x=.001623; for one ton .001623×2000=3.246 lbs.= resistance per ton.

Experiment 10.—At same place as 8 and 9. Engine 63, weight 60 tons; cars 16 to 30, weight 360 tons; total, 420 tons; length of train, 533 feet.

Time passing base (300)	30.4 sec.
Speed per second	9.87 ft.
Speed per minute	592 ft.
Speed per hour	6.73 m.
Run, north	1,013 ft.
Time, actual	185.5 sec.

Time estimated as 205 seconds. Light wind on side and toward rear.

V=9.87 S=1,013

T (actual)=185.5.

$$F=\frac{V^2}{2S}=.04852$$

$$F=\frac{2S}{T^2}-.05888$$

$$F=\frac{V}{T}=.0532$$

1.16061

Average, 0.5353

1 : 32 :: x : .05353. x=.001673; for 1 ton=.001673 × 2000=3.346 lbs.=resistance per ton.

Experiment 11.—Same place, time and train. Same conditions, except that train moved south over same ground, the engine being in rear, and same light wind tending to retard, if enough in rear (front) to have any effect.

Time passing base (300)	33.8 sec.
Speed per second	8.87 ft.
Speed per minute	532.2 ft.
Speed per hour	6.04 m.
Run	592 ft
Time, actual	132.5 sec.
" estimated	133.5 "

V=8.87 S=592 T=132.5

$$F=\frac{V^2}{2S}=.06645$$

$$F=\frac{2S}{T^2}=.06744$$

$$F=\frac{V}{T}=.06694$$

.20063

Average, .06688

1 : 32 :: x : .06688. x=.00209; per ton=.00209×2000=4.18 lbs. = resistance per ton.

Experiment 12.—Same time, place, train and conditions, except run, which was north, as in 10.

Time passing base (300 feet)	27 sec.
Speed per second	11.11 ft.
Speed per minute	666.66 ft.
Speed per hour	7.57 m.
Run	1,079 ft.
Time	182.4 sec.
Time (estimated)	194.2 "

V=11.11 S=1,079 T=182.4

$$F=\frac{V^2}{2S}=.0572$$

$$F=\frac{2S}{T^2}=.0648$$

$$F=\frac{V}{T}=.0609$$

.1829

Average, .061

1 : 32 :: x : .061. x=.001906; per ton =.001906×2000=3.81 lbs. = resistance per ton.

Mem.—Had estimated time been used we would have 3.58.

Experiment 13.—Same place, time and train. Run south.

Time passing base	26.8 sec.
Speed per second	11.19 ft.
Speed per minute	671.4 "
Speed per hour	7.63 m.
Run	925 ft.
Time	166.4 sec.
" (estimated)	165.3 "

V=11.19 S=925 T=166.4.

$$F=\frac{V^2}{2S}=06768$$

$$F=\frac{2S}{T^2}=.06681$$

$$F=\frac{V}{T}=.06725$$

.20174

Average, .06725

1 : 32 : : x : .06725. x=.0021016; for one ton, .0021016×2000=4.20 lbs.

Experiment 14.—On low grade. Same train as in 13. Running north. Base line 200 feet. From south end of base line grade is .377 per 100 feet on an average=19.905 feet per mile. Back of south end of base line is level.

Time passing base (200 feet)	11.3 sec.
Speed per second	17.86 ft.
Speed per minute	1,071.4 ft.
Speed per hour	12.17 m.
Run	847 ft
Time	104 sec.
" (estimated)	94.3 sec.

Wind on right side light, and a little in front.

To get at the average resistance on the basis of a uniform retardation as in the first experiments, we have:

V=17.86 S=847 T=104

$$F=\frac{V^2}{2S}=.1883$$

$$F=\frac{2S}{T^2}=.1566$$

$$F=\frac{V}{T}=.1717$$

.5166

Average, .1722

1 : 32 : : x : .1722; x=.005381; per ton, .005381×2000=10.76 lbs.

Train.	Tons.	Distance on grade.	Tons × distance.
Engine	36.6	847	
Tender	23.4	847	
Cars 30	23.8	847	
" 29	24.7	847	156,779.7
" 28	25.5	847	
" 27	25.6	847	
" 26	24.5	847	
" 25	24.2	835	20,207.0
" 24	24.0	803	19,272.0
" 23	24.6	781	19,212.6
" 22	22.0	749	16,478.0
" 21	24.0	717	17,208.0
" 20	23.5	685	16,097.5
" 19	23.9	653	15,603.7
" 18	23.9	621	14,841.9
" 17	24.1	589	14,194.9
" 16	21.8	557	12,142.6
	420.1		322,040.9

322,049.9×7.54 (due gravity) ... 2,428,188.386

Now take the estimated resistance per ton above, and the weight 420.1×10 76×847 (distance) ... 3,828,673.772

Other than gravity ... 1,400,485.386

Divide by 847 (distance) ... 1,653.406

Divide again by 20 1 (tons), we have ... 8.935 lbs.

equals resistance other than gravity.

Experiment 15.—Same time, place and train. Light wind in front and right side.

Time passing base (200)	8.2 sec.
Speed per second	24.39 ft.
Speed per minute	1,463.4 "
Speed per hour	16.63 m.
Run	1,983 ft.
Time	148.5 sec.
" (estimated)	121 "

As before,

V=24.39 S=1,982 T=148.5

$$F=\frac{V^2}{2S}=.15008$$

$$F=\frac{2S}{T^2}=.17976$$

$$F=\frac{V}{T}=.16424$$

.49408

Average, .16469

1 : 32 : : x : .16469. x=.0051465; per ton=.0051465×2000=10.293 lbs., equals total resistance per ton, *if* resistance were constant. The grade for 1,000 ft. from north end of base line was, as before, .377 per 100 ft., and beyond that was level, as in 14.

Train.	Tons.	Distance on grade.	Tons × distance.
Engine	36.6	1,000	36,600.0
Tender	23.4	1,020	23,868.0
30	23.8	1,052	25,037.6
29	24.7	1,084	26,774.8
28	25.5	1,116	28,458.0
27	25.6	1,148	29,388.8
26	24.5	1,180	26,460.0
25	24.2	1,200	
24	24.0	1,200	
23	24.6	1,200	
22	22.0	1,200	
21	24.0	1,200	
20	23.5	1,200	283,200.0
19	23.9	1,200	
18	23.9	1,200	
17	24.1	1,200	
16	21.8	1,200	
	420.1		479,787.2

479,787.2 × 7.54 (due gravity)	3,617,595.488
As before,	
420.1 (tons) × 10.293 (estimated resistance) × 1,982	8,570,343.993
	4,952,749 505
Divide by 1,982 (distance)	2,498.864
Divide by 420.1 (tons)	5,948 lbs.

resistance per ton other than gravity.

Experiment 16.—Same time, train and conditions as last. Light wind in front and right side.

Time passing base (200)	11.4 sec.
Speed per second	17.54 ft.
Speed per minute	1,052.6 "
Speed per hour	11.96 m.
Run	870 ft.
Time	103 sec.
" (estimated)	99 "

As before,

$V=17.54 \quad S=870 \quad T=103$

$$F=\frac{V^2}{2S}=.17681$$

$$F=\frac{2S}{T^2}=.16401$$

$$F=\frac{V}{T}=.17029$$

.51111

Average, .17037

1 : 32 : : x : .17037. x=.005324; .005324 × 2000=10.648=estimated resistance if regularly applied.

Train.	Tons.	Distance on grade.	Tons × distance.
Engine	36.6	870	
Tender	23.4	870	
30	23.8	870	
29	24.7	870	160,167.0
28	25 5	870	
27	25.6	870	
26	24.5	870	
25	24.2	838	20,279.6
24	24.0	806	19,344.0
23	24.6	774	19,040.4
22	22.0	742	16,324.0
21	24.0	710	17,040.0
20	23.5	678	15,933.0
19	23.9	646	15,439.4
18	23.9	614	14,674.6
17	24.1	582	14,026.2
16	21.8	550	11,990.0
	420.1		324,258 2

324,258.2 × 7.54, (due gravity)	2,444,906.828
As before,	
420.1 (wt.) × 10.648 (est. resist.) × 870 (dist.)	3,891,705.576
Leaving a resistance other than gravity	1,446,798.748
Divide by 870 (distance)	1,662.987
Divide by 420.1 (tons)	3.878 lbs.

equals resistance per ton other than due gravity.

Experiment 17.—Same train, time and conditions:

Time passing base	12.8 sec.
Speed per second	15.62 ft.
Speed per minute	937.5 "
Speed per hour	10 05 m.
Run	670 ft.
Time	93 sec.
" (estimated)	86 "

$V=15.62 \quad S=670 \quad T=93.$

$$F=\frac{V^2}{2S}=.18207$$

$$F=\frac{2S}{T^2}=.15496$$

$$F=\frac{V}{T}=.16796$$

.50499

Average, .16833

1 : 32 : : x : .16833. x=.00526; .00526 × 2000 = 10.52 lbs.=estimated constant resistance.

Train.	Tons.	Distance on grade.	Tons × distance.
Engine	36.6	670	
Tender	23.4	670	
30	23.8	670	
29	24.7	670	123,950.0
28	25.5	670	
27	25.6	670	
26	24.5	670	
25	24.2	638	15,439.6
24	24.0	606	14,544.0
23	24.6	574	14,120.4
22	22.0	542	11,924.0
21	24.0	510	12,240.0
20	23.5	478	11,233.0
19	23 9	446	10,659 4
18	23.9	414	9,894.6
17	24.1	382	9,206.2
16	21.8	350	7,630.0
	420.1		240,841.2

240,841.2 × 7.54 (due gravity)....	1,815,942.648
As before, 420.1 (tons) × 10.52 (est. resist.) × 670 dist.)	2,961,032.840
Resistance other than gravity ...	1,145,190 192
Divide by 670 (distance).	1,709.239
Divide by 420.1 (tons)	4,068 lbs.
resistance other than due Gravity.	

Experiment 18.—Same train, place and conditions. Wind as in last, about three miles or, perhaps, more per hour on right hand and well ahead.

Time passing base (200).....	25.6 sec.
Speed per second.....	7.81 ft.
Speed per minute.....	468.7 "
Speed per hour.....	5.32 m.
Run	191 ft.
Time.....	53 sec.
" (estimated).....	49 "

$V=7.81 \quad S=191 \quad T=53$

$$F=\frac{V^2}{2S}=.1592$$

$$F=\frac{2S}{T^2}=.1360$$

$$F=\frac{V}{T}=.1474$$

.4426

Average, .1475

1 : 32 : : x : .1475. x=.0046; .0046 × 2000=9.22 lbs.=estimated constant resistance.

Train.	Tons.	Distance on grade.	Tons × distance.
Engine.....	36.6	191	
Tender.....	23.4	191	
30.....	23.8	191	
29.....	24 7	191	35,163.1
28.....	25.5	191	
27.....	25.6	191	
26.....	24.5	191	
25	24.2	159	3,847.8
24.....	24.0	127	3,048.0
23.....	24.6	95	2,337.0
22.....	22.0	63	1,386.0
21..	24.0	31	744.0
20.....	23.5		
19.....	23.9		
18.....	23.9		
17.....	24.1		
16.....	21.8		
	420.1		46,525.9

46,525.9 × 7.54 (due gravity).....	350,805.286
420.1 × 9.22 (est. resist.) × 191 distance.....	739,8[illegible]4.502
Resistance other than gravity.....	488,999.216
Divide by 191 (distance).....	2,560.206
Divide by 420.1 (tons).....	6.094 lbs.
resistance per ton other than gravity.	

Experiment 19.—Same train, etc.

Wind a little stronger each time; three miles an hour at least.

Time passing base.	26.6 sec.
Speed per second.....	7.51 ft.
Speed per minute.....	450.14 "
Speed per hour.....	5.11 m.
Run	179 ft.
Time.....	51 sec.
" (estimated).....	49 "

$V=7.51 \quad S=179 \quad T=51.$

$$F=\frac{V^2}{2S}=.15754$$

$$F=\frac{2S}{T^2}=.13755$$

$$F=\frac{V}{T}=.14675$$

.44134

Average, .14711

1 : 32 : : x : .14711. x=.004597; .004597 × 2000=9.094 lbs.= estimated resistance if constant.

Train.	Tons.	Distance on grade.	Tons × distance.
Engine.....	36.6	179	
Tender.....	23.4	179	
30.....	23.8	179	
29.....	24.7	179	32,953.9
28.....	25.5	179	
27.....	25.6	179	
26.....	24.5	179	
25.....	24.2	147	3,557 4
24.....	24.0	115	2,760.0
23.....	24.6	83	2,041.8
22.....	22.0	51	1,122.0
21.....	24.0	19	456 0
20	23.5		
19.....	23.9		
18.....	23.9		
17	24.1		
16.....	21.8		
	420.1		42,891 1

42,891.1 × 7.54 (due gravity)	323,338,894
42,891.1 × 9.094 (est. resistance) × 179 (dist.)	683,849.703
	360,450.809
Divide by 179 (dist.)	2,013.687
Divide by 420.1 (tons)	4.793 lbs.

resistance other than due gravity per ton.

Experiment 20.—Same time, train, etc.

Time passing base	20.8 sec.
Speed per second	9.61 ft.
Speed per minute	577 "
Speed per hour	0.56 m.
Run	305 ft.
Time	62 sec.
" (estimated)	63 "

$V=9.61 \quad S=305 \quad T=62$

$$F=\frac{V^2}{2S}=.151397$$

$$F=\frac{2S}{T^2}=.158688$$

$$F=\frac{V}{T}=.155000$$

.465085

Average, .155028

1 : 32 : : x : .155028. x=9.689 lbs.= estimated constant retarding power per ton.

Train.	Tons.	Distance on grade.	Tons × distance.
Engine	36.6	305	
Tender	23.4	305	
30	23.8	305	
29	24.7	305	56,150.5
28	25.5	305	
27	25.6	305	
26	24.5	305	
25	24.2	273	6,606.0
24	24.0	241	5,784.0
23	24.6	219	5,387.4
22	22.0	187	4,114.0
21	24.0	155	3,720.0
20	23.5	123	2,890.5
19	23.9	91	2,174.9
18	23.9	59	1,410.1
17	24.1	27	650.7
16	21.8		
	420.1		88,888.7

88,888.7 × 7.54 (due gravity)	670,220.798
420.1 × 9.689 (estimated resistance) × 305 (distance)	1,241,456.414
Total resistance due other than gravity	571,235.616
Divide by 305 (distance)	1,872.903
Divide by 420.1 (tons)	4 458 lbs.

equals resistance per ton other than gravity.

Experiment 21.—Same train, time and conditions.

Time passing base	17.6 sec.
Speed per second	11.37 ft.
Speed per minute	662 "
Speed per hour	7.73 m.
Run	405 ft.
Time	70 sec.
" (estimated)	71 "

$V=11.37 \quad S=405 \quad T=70.$

$$F=\frac{V^2}{2S}=.1596$$

$$F=\frac{2S}{T^2}=.1653$$

$$F=\frac{V}{T}=.1624$$

.4873

Average .16243

1 : 32 : : x : .16243. x=.005107; .005107 × 2000=10.214 lbs.= estimated constant resistance per ton.

Train.	Tons.	Distance on grade.	Tons × distance.
Engine	36.6	405	
Tender	23.4	405	
30	23.8	405	
29	24.7	405	74,560.5
28	25.5	405	
27	25.6	405	
26	24.5	405	
25	24.2	373	9,026.6
24	24.0	341	8,184.0
23	24.6	309	7,601.4
22	22.0	277	6,094.0
21	24.0	245	5,880.0
20	23.5	213	5,005.5
19	23.9	181	4,325.9
18	23.9	149	3,561.1
17	24.1	117	2,819.7
16	21.8	85	1,853.0
	420.1		128,911.7

128,911.7×7.54 (due gravity)....	971,994.218
420.1×10.214 (est. resistance) × 405 (dist).....	1,737,815.067
Resistance other than gravity....	765,820.849
Divide by 405 (distance).....	1,644.002
Divide by 420.1 (tons).....	3.913 lbs.

resistance per ton other than gravity.

Experiment 22.—Same train and conditions.

Time passing base.....	16.4 sec.
Speed per second.....	12.2 ft.
Speed per minute.....	732 "
Speed per hour.....	8 3 m.
Run	415 ft.
Time.....	70.5 sec.
" (estimated).....	68 "

$V=12.2 \quad S=415 \quad T=70.5$

$$F=\frac{V^2}{2S}=.17932$$

$$F=\frac{2S}{T^2}=.16703$$

$$F=\frac{V}{T}=.17305$$

.51937

Average, .17312

$1 : 32 : : x : .17312. \quad x = .00541;$
$.00541 \times 2000 = 10.83$ lbs. = estimated constant resistance per ton.

Train.	Tons.	Distance on grade.	Tons × distance.
Engine.....	36.6	415	
Tender.....	23.4	415	
30.....	23.8	415	
29.....	24.7	415	76,401.5
28.....	25.5	415	
27.....	26.6	415	
26.....	24.5	415	
25.....	24.2	383	9,268.6
24.....	24 0	351	8,424 0
23.....	24.6	319	7,847.4
22.....	22 0	287	6,314.0
21.....	24.0	255	6,120.0
20.....	23.5	223	5,240.5
19.....	23.9	191	4,564 9
18.....	23.9	159	3,800.1
17.....	24.1	127	3,060.7
16.....	21.8	95	2,071.0
	420.1		133,112.7

133,112.7×7.54 (due gravity)....	1,003,669.758
420.1×10.82 (est. resist.) × 415 (dist.).....	1,886,375.030
Resistance other than gravity....	883,705.272
Divide by 415 (distance).....	2,129.410
Divide by 420.1 (tons).....	5.068 lbs.

resistance per ton other than due gravity.

See Summary of Experiments next page.

A portion of the resistance other than gravity in these experiments was atmospheric resistance, partly due to speed of trains and partly due to wind.

In experiments 1 to 6 and 8 to 9 the wind was so light and so near right angles that its effect could hardly be estimated.

In 10 and 12 it was on the rear quarter, 30 or 40 degrees from the line of the train and about four miles per hour.

In the other experiments, when it is mentioned it was on the right hand, 30 to 40 degrees from dead ahead, and estimated about four miles per hour.

In order to make our estimates we will assume that the pressure per square foot due speed is as laid down by various authorities and as given below:

Speed per hour.		Velocity per second.	Pressure per square foot.
1	.....	....	....
2	.....	....	....
3	.....	4.4	.044
4	.....	5.87	.079
5	.....	7.83	.123
6	.....	8.8	.177
7	.....	10.25	.241
8	.....	11.75	.315
9	.....	13.2	.400
10	.....	14.67	.492
12	.....	20.5	.964
15	.....	22.	1.107
16	.....	23.45	1.25
18	.....	26.4	1.55

The train consisted of an engine and fifteen coal cars, loaded as high as coal could well be piled on.

The surface presented by the engine we estimated to be about 64 square feet, and as the wind was at an angle, it took effect on the sides of the train as well as upon the ends of the cars which presented a surface, and the coal, and we estimated that the surface so acted on was about six square feet per car. This would make the whole surface 154 square

SUMMARY OF EXPERIMENTS.

No.	Speed per hour.	Started on		Run on		Distance.	Time.	Est. time.	Resist. due gravity.	Other resist.
		Grade.	Curve.	Grade.	Curve.					
1	11.70	level.	....	52.8	....	538 ft.	..	63 sec.	20 lbs.	5 98
2	9.43	"	....	"	....	403 "	..	58.3 "	"	6.45
3	6.81	"	....	"	....	234 "	..	47 "	"	8.09
4	7.70	"	....	"	....	316 "	..	56 "	"	5.95
5	10.65	"	....	"	....	495 "	..	63 "	"	5.38
Av'ge.	9.25	.....	....	....	....	397 ft.	..	57.4 sec.	20 lbs.	6.37
6	11.33	52.8	....	52.8	2°	321 "	..	39 "	20 "	6.55

No.	Speed per hour.	Started on		Run on		Time.		Distance run.	Resistance.		
		Grade.	Curve.	Grade.	Curve.	Actual	Est'd.		Gra'y.	Other.	
8	12.08	Level	..	Level	..	..	116.5	1,500	..	5.19	r'nning at speed 5.52 end of 1,500 feet.
9	7.82	"	..	"	..	..	221	1,272	..	3.246	
10	6 73	"	..	"	..	185.5	205	1,013	..	3.346	
11	*6.04	"	..	"	..	132.5	133.5	582	..	4.18	going south
12	7.57	"	..	"	..	182.4	194.2	1,079	..	3.81	
13	7.63	"	..	"	..	166.4	165.3	925	..	4.20	" "
Av'ge.	7.98	..	..	..	..	..	..	..	..	3.995	.....
"	7.16	..	..	..	..	..	..	976	..	3.756	9 to 13
14	12.17	..	..	19.9	..	104	94.3	847	7 54	3.935	
15	16.63	..	..	"	..	148.5	121	1,982	"	5.948	
16	11.96	..	..	"	..	103	99	870	"	3 878	
17	10.65	..	..	"	..	93	86	670	"	4.068	
18	5.32	3-5 l'l	..	"	..	53	49	191	"	6.094	
19	5 11	2-5 up	..	"	..	51	49	179	"	4.793	
20	6.56	grade	..	"	..	62	63	305	"	4.458	
21	7.73	19.9	..	"	..	70	71	405	"	3.913	
22	8.30	..	..	"	..	70.5	68	415	"	5 068	
	9.38	..	..	..	..	83 8	..	651.6	7.54	4 684	Average.
						8 to 22				4.408	Total.
Average all on straight line 1-5, 8-22										4.898	
No 6 on two degrees curve										6.55	

feet, and upon this surface we have figured, although Mr. Chanute estimates that the additional surface for each car is about twenty per cent. of the first surface presented, or a little more than twice what we have taken. If he is right, results 67 per cent. greater than we show would be reached.

As we do not wish to *over*-estimate, we will assume our figures for 154 square feet for wind pressure.

The wind pressure varies as the square of the velocity; as our train moved from a given speed to a standstill we must average the effect, which would be as the average of the square of the various speeds during the time—which within the limits of our experiments would be substantially one-fourth less than half the maximum effect, or three eighths of the maximum effect, *i. e.*, maximum pressure. The variation from this is so small that it will make no practical difference in figures.

On this basis we figure as follows:

EXPERIMENT 1.

Pressure of wind	.0	
" due maximum speed. ...	.964	lbs.
Three-eighths of this	.362	"
154 sq. ft. × .362 lbs. pressure	55.748	"
Divide by 420.1 tons (wt. of train).	.132	"
resistance per ton due atmospheric causes.		

[In the same way the writer gets the atmospheric resistance for each of the 24 experiments. It is not thought essential to print the calculations here.]

From these figures we may conclude that the frictional resistance of *that train*, including the engine, was 4.725 lbs. per ton, though many of the results are below this.

You will note that in five cases the estimated time taken in the stop was more than the actual, and in eight cases it was less.

You will note that the initial speed in cases where the estimated time exceeds the actual is low, averaging 6.72 miles; while in cases where the estimated time is less than the actual, the initial speed is higher, averaging 9.72 miles, or almost 45 per cent. higher.

Now, in the first ten cases the average initial speed is on an average of 9.69 miles, and, if the same rules hold, we will have an estimated time of considerably *less* than the actual.

In the case of No. 12 we found that an increase of six per cent. of the estimated time over the actual would, in calculation, show a six-per-cent. decrease in the resistance.

If the time used in the calculation of cases 1 to 9 is too small, then the use of the actual or correct time in the calculation would show a resistance less

RESULTS TABULATED.

Series.	Experiments.	Resistance other than gravity.	Atmospheric resistance.	Friction and other resistance.
1	1	5.98	.182	5.848
	2	6.45	.062	6.388
	3	8.09	.091	8.009
	4	5.95	.030	5.920
	5	5.38	.090	5.290
Average				6.291
2	6	6.55	.110	6.440 on 2° curve.
3	8	5.190	.204	4.986
	9	3.246	.044	3.202
	10	3.346	.002	3.344
	11	4.180	.056	4.124
	12	3.810	.026	3.784
	13	4.200	.069	4.131
Average				3.929
4	14	3.935	.163	3.772
	15	3.948	.215	3.733
	16	3.878	.162	3.716
	17	4.068	.130	3 938
	18	6.094	.049	6 045
	19	4.793	.047	4.746
	20	4.458	.059	4.399
	21	3.913	.070	3.843
	22	5.068	.079	4.989
Average				4.353
Average 8 to 22				4.183
1 to 5 and 8 to 22				4.710

TABLE SHOWING ACTUAL TIME, ESTIMATED TIME, INITIAL VELOCITY IN MILES PER HOUR, AND FRICTIONAL RESISTANCES AS CALCULATED.

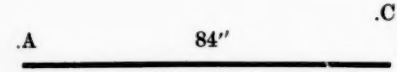
Series.	Experiments.	Observed time.	Estimated time.	Initial speed.	Frictional resistance.
	1	....	63	11.70	5.848
	2	....	58.3	9.43	6.388
1.....	3	....	47.	6.81	8.309
	4	....	56.	7.70	5.920
	5	....	63.	10.65	5.290
2.....	6	....	39.	11.33	6.440 on 2° curve.
	8	....	116.5	12.08	4.986
	9	....	221.	7.82	3.202
	10	185.5	205.	6.73	3.344
3.....	11	132.5	133.5	6.04	4.124
	12	182.4	194.2	7.52	3.784
	13	166.4	165.3	7.63	4.131
	14	104	94 3	12.17	3 772
	15	148.5	121.	16.63	3.733
	16	103	99	11.96	3 716
	17	93	86	10.65	3.938
4.....	18	53	49	5.32	6.045
	19	51	49	5.11	4.746
	20	62	63	6.56	4.399
	21	70	71	7.73	3.843
	22	70.5	68	8.30	4.989
Average.....	.....	.....	.....	9.041	

than that tabulated by a percentage equal to the error in the time element.

How much of the resistances calculated is due to imperfect track is unknown. In no case was the track perfect, though it was fair, and an average sand-ballasted track.

Some very crude experiments were made to test the frictional resistance both at the start and when in motion, the cars used being of the lot used in former experiments, weight of which we had.

Our arrangement for measuring the resistance being a lever with length four inches and eighty-four inches, with spring balance to measure strain; thus:



This lever being upon one empty coal car at one end, the point A being fixed upon the car, the point B being upon a wire which was fastened to the draw-bar of the car to be tested; the point C being the point to which the spring balance was fixed, the other end of the balance being put to the lever.

We found that the arrangement was not accurate, as, for instance, the weight of the parts would show on the balance, and we had to start in by an allowance to provide against such influence. The pointer would bob about rapidly, so that an exact reading could hardly be taken. We attempted to read the amount that it took to start the car, and then how much to keep it going. We could not get an exactly steady motion, and the strains after the start were spasmodic in spite of all we could do.

The point selected was upon a level piece of track, on straight line, steel rail and good surface.

The first test was with car 2,057, weight 47,700 lbs.

We give, first, the number of the test; second, the amount our scale registered (after deducting our allowance) to start the car; third, the range through which our pointer vibrated (after deducting as before) to keep the car going:

No. Test.		Pointers, when car started.	Range of pointers to keep moving.
1		29	4–12
2		24	2–10
3		24	4– 8
4		30	4– 8
5		14	4– 8
6		30	4–10
7		12	3– 6
8		14	3–10
9		13	4– 8
10		16	4–10
11		11	....
12		8	2– 6
13		8	2– 5
14		12	2– 6
15		11	....
16		13	....
17		16	....
18		10	....
19		15	....
20		21	....
21		15	....
22		20	....
23		10	....

Footing of experiment to start 355
Average..... 15.44
Multiply by the leverage, 21, and we have..... 324.24
Divide by 25.85 (tons)..... 13.595

which we may take as the resistance per ton upon this car at the start.

Foot the average of amounts required to keep the car in motion, and we have..... 745
Wh'ch gives an average of..... 5.73
Multiply by 21 (leverage)..... 120.33
Divide by 23.85 (tons)..... 5.04
equals resistance per ton.

The wind was light and upon side, so did not affect it.

Another car was taken, weight, 47,100 lbs.=23.55 tons.

The result as a whole was very unsatisfactory, and of little or no value, except to show that the frictional resistance from a start is much more than while in motion.

The average resistance to a start was 17.27 lbs. per ton, while that to keep in motion seems to have been about 5.10 lbs. per ton, or less than one-third of the former.

As before,

Test.		Start.	Range
24		21	....
25		21	....
26		24	....
27		21	....
28		27	4– 8
29		12	8–12
30		8	4– 8
31		19	3– 8
32		17	7–14
33		15	2–10
34		14	6–14
35		19	....
36		26	....
37		28	....
38		30	....
39		28	2–12
40		27	....
41		28	....
42		19	8–12
43		31	4–10
44		27	....
		462	78
Av'ge.		22	7.8

22×21=462, divided by 23.55=19.62 =average resistance per ton to starting.

7.8×21=163.8, divided by 23.55=6.96 =resistance when running very slowly.

Next car: weight, 50,700.

Test.		Start.	Range.
45		31	....
46		28	4–10
47		26	2– 6
48		34	4–12
49		30	3– 6
50		31	3– 8
		180	30
Av'ge		30	6

30×21 (leverage)=630, divided by 25.35=24.85 average resistance at start.

6×21=126, divided by 25.35=4.97= resistance in motion.

The motion in last car was about three or three and a half miles per hour, while in other two it was very much less; just barely a motion.

THE PERMISSIBLE STRAIN UPON BRICKWORK.

By Dr. BÖHME.

Mittheilungen aus der "Prüfungsstation für Baumaterialien."

Translated for Abstracts of the Institution of Civil Engineers.

The experiments given in detail in this paper were made to ascertain the crushing strength of brickwork composed of different kinds of brick with various mortars. The mortars used were made with either lime or Portland cement, in each case mixed with sand. The bricks were those chiefly in use in Berlin. The composition of the mortar was found to have great influence upon the strength. Brickwork set in a mortar composed of 1 part lime and 2 parts sand possessed only 44 per cent. of the strength of the bricks alone, while the mortar composed of 1 part Portland cement and 3 parts sand raised the percentage to 63. A great number of tests were made, full particulars of each being given. The results obtained are summarized in the following table, in which the safety limit is taken as one-tenth of the absolute crushing strength.

Description of Brick.	Average Crushing Strength of Brick Alone.	Permissible Strain upon Brickwork with Mortar composed of—			
		1 Lime. 2 Sand. 4.4 per cent.	7 Lime. 1 Cement. 16 Sand. 4.8 per cent.	1 Cement. 6 Sand. 5.5 per cent.	1 Cement. 3 Sand. 6.3 per cent.
	Lbs. per Square Inch.	Lbs. per Square Inch.			
Ordinary stocks.....	2,930	129	139	161	185
Selected "	3,669	162	176	202	232
Clinkers.....	5,390	237	259	296	341
Porous Bricks.....	2,617	115	125	144	165
Porous perforated do.	1,195	53	57	65	75
Perforated do	2,759	121	132	152	171

The figures arrived at by the author are in each case higher than those upon which the building regulations of Berlin are based.

Russian Mineral Resources.—The Russian people appear to have some cause for being dissatisfied with the way in which the rich resources of their own country are being neglected. At the racent conference of the ironmasters of Poland, at Warsaw, a petition was adopted praying the Government to revise the traffic rates of the principal Polish lines, and alleging that a ton of English rails can be conveyed from England to Warsaw as cheaply as a ton of Polish rails from the Radom and other districts of West Russia. During the conference it was mentioned that the firm of Krampst & Co. had realized a clear profit of £44,350 during the past year, enabling it to declare a dividend of 9 per cent. It was also asserted that, while the neighboring iron mines of Silesia are becoming poorer every year, and do not yield more than 28 per cent. of iron, the yield of the Russian mines of Poland ranges between 30 and 45 per cent., in consequence of which, foreign firms have applied for upwards of 100 plots of ground in the Dombrova district, in order to be able to extract the ore and dispatch it to Silesia. In the Krivoy Rog district, of South Russia, the yield of magnetic ore is stated by a correspondent to be 65 per cent., and a French company is working the deposits.

VENTILATION OF SEWERS.*

By GEORGE EEDES EACHUS, M. I. C. E.

From "The Architect."

In presenting a paper on the ventilation of sewers for the consideration and discussion of the Society, the author has confined himself to giving a short account of what has been done, and a description of a system which he has personally adopted as the result of investigation into the working and defects of previous systems.

Until the last twenty or thirty years it was not the general custom to ventilate sewers, and most of the main sewers themselves were constructed without manholes or ventilators, the branch sewers often discharging into cesspools, the overflow from which was connected with the main sewers—the cesspools being as far as practicable, hermetically sealed and seldom emptied, the consequence being that the stagnant sewage was decomposed, and the germs of disease rapidly spread.

The numerous cases of typhoid and kindred diseases arising from these defects in works of drainage caused sanitary reformers to awake to the dangers of pent-up sewage, and it was then sought to send the sewage away as far and as quickly as possible, and outlets were provided for the escape of the sewer gas both from cesspools and sewers.

The modern French system has been based somewhat on this system, the cesspools themselves being retained, but made as air and watertight as possible, with upcast shafts to take the sewer gas from the cesspools, and the soil-pipe carried down from the top of the house, with an outlet to the air at the top, and the bottom extended below the surface level of the liquid retained in the cesspool as a trap. This system, called the *fosse fixe*, is sometimes connected with a sewer, sometimes not, and of course, especially in the latter case, involves periodical and frequent cleansing, and this necessity has caused the invention of several ingenious arrangements for emptying the *fosse fixe*, or cesspool; but this system is much more costly than the English, owing to the large annual cost both of cleansing the cesspools and carting away the soil, otherwise the effect where the work is scientifically and well executed, and the emptying and cleansing constantly attended to, is to keep the sewer gas from being discharged within the dwelling. But much more sewer gas is generated than in the English system of constant flow, and the annual cost is about five times the cost of removal by water, or about 12s. 6d., as compared with 2s. 6d. per head per annum in the case of large towns.

The main sewers in France, and generally abroad, are, as a rule, very badly ventilated, and their construction is often such as to favor the generation of large quantities of sewer gas, discharged often by gullies and other openings immediately under the nose of foot passengers.

In England the sewers, as far as their liability to generate sewer gas is concerned, have been generally much better constructed than abroad, and of much smaller dimensions than the French, and better proportioned to the normal flow of sewage, filling, consequently, at all times, a larger portion of the sewer, and the sewage runs with a greater and, for the sewers, more self-cleansing velocity. Sewers so constructed also expose a smaller proportionate surface of sewer wall to the alternate wetting by sewage and drying and decomposition which takes place, especially at certain seasons of the year, and when dry weather follows after a storm. The English sewers are better constructed to avoid generation of sewer gas, and better cleased, but, until recently, very little has been done in the way of ventilating the sewers except by the insertion of a greater or less number of stink outlets, and next to nothing has been done to render sewer gas escaping at these blowholes innocu-

* A paper read at a meeting of the Civil and Mechanical Engineers' Society.

ous, nor until recently has much been done to assist in the cleansing of sewers in a practical manner. It is needless to inform engineers that it is absolutely necessary to keep sewers clean, either by natural or artificial flushing, and for this purpose, where a sewer is laid with a flat gradient, much more is needed than an occasional flush at a few points from a 500 or 1,000 gallons self-acting flush-tank, the flush from which, in a sewer of moderate size, is spent in a very short distance. Most sewerage engineers who have turned their attention to the ventilation of sewers have contented themselves with making the stink outlets, or ventilating manholes, at intervals of from about 60 yards and upwards, the distance generally adopted being over 100 yards, the idea being that the sewer gas discharging at these outlets would be sufficiently diluted with atmospheric air to render it innocuous. The author's own practice has been, of late, to place ventilating manholes 200 ft. apart as a convenient distance for examination, and quite far enough for the free circulation of air, which the author seeks to secure in every sewer.

As the age of sewers has increased, all the defects due to defective private-house drainage and connections of house drains with the sewers have become apparent, and the smells from some ventilating manholes have so increased, that public opinion has been roused even to the extent of compelling the stoppage of ventilators, instead of adding to their number; the public generally, in a time of excitement and fear of microbes and bacilli, acting upon the principle that what the eye does not see or the nose smell, the heart does not grieve at.

When ventilators were first introduced, many attempts were made to filter the sewer gas before allowing it to discharge into the open air, and many and complicated were the ventilators patented, charcoal being generally the substance employed to filter the gas; but owing partly to the charcoal being often so displaced, when the filter was put in, that the sewer gas could pass freely through the open spaces, partly owing to the ventilators being so constructed that the charcoal was constantly wet and clogged, so as not to act properly, owing to these and other causes, these charcoal trays and filters have gradually come into disuse.

Various attempts have also been made to create strong upcast draft by furnace chimneys, cowls, or other artifical means, but these attempts have never been more than locally, and then only very partially successful.

The author having himself encountered most of these practical difficulties, tried a number of experiments at his own house and on sewers under his charge, in order to test by the practical and elementary sense of smell (which is the test put by the public, rather than by delicate anemometers and other means, when and under what conditions the sewer gas was most freely generated and discharged. The next point was to ascertain how the flow of sewer gas could be so regulated as to prevent undue accumulation at any one point, and how the passers by the ventilators could be relieved of the constantly unpleasant smell experienced at many places. The result of these experiments was to show that a well-constructed sewer, with a moderately quick fall and constant flow, running from one-third to one-half full, or more, of sewage, generated the least quantity of sewer gas; also that a sewer similarly constructed, with a similar fall, seemed to form the best channel for the conveyance of sewer gas; and for this reason, where practicable, the author of this paper prefers to have a sloping outlet, up which the sewer gas may ascend, so as to arrive at its point of discharge with the least possible obstruction. More sewer gas was found to be generated in the larger and badly-constructed sewers, especially in those with flat fall and varying flow, and where the velocity of flow is insufficient for the sewer to be self-cleansed. The sense of smell is, of course, most apparent when the necessary conditions of stagnation and comparative temperatures of the sewer and of the air happen to meet, and hot fluids entering the sewers always add to the aroma and increase the difficulty of dealing with the gas.

The flow of the gas and the sense of smell both seem to travel, as a rule, in the direction inverse to the flow of sewage, but this rule is by no means without exceptions. For the purposes of this paper it is, however, better to deal with

the general rule, which is, that sewer gas, when generated, travels upward, and tends to accumulate in the upper districts at points where there is some check to the flow, or some local circumstance to make a ventilating manhole a good upcast shaft. The author had a good case in point where a clean, new, well-laid sewer, with a rather quick fall and no sewage in it, served as an admirable ventilating shaft for an old sewer.

To prevent this accumulation of sewer gas—to dilute the gas with atmospheric air, and to filter the diluted gas before its discharge—the author, in conjunction with Mr. Maignen (the well-known patentee of the *Filtre rapide*), has taken out a patent for localizing the sewer gas, diluting it as much as possible, and filtering it through a charcoal filter in such a way as to overcome the disadvantages above referred to. The accumulation is prevented by localization, and this is effected by means of double or divided manholes provided with stops or valves, placed at the lower part of the middle division in each manhole. The model and drawing show the general arrangement by which the air is allowed to enter the sewer through that part of the manhole called the air inlet. The air passes up the line of sewer, mixing with the sewer gas as it travels, until it comes to the valve or stop at the next manhole, which prevents its further progress, and turns it through the filter out at the ventilating cover or gas outlet. This valve or stop is made in sections, being like a balance valve on a common spindle, and each of the lower sections working independently of those above, so that, while the sewage flows down the sewer, the flow of gas in the opposite direction is arrested and diverted up the gas outlet at each manhole.

In trying to ascertain the velocity and force with which sewer gas travels, the author has not yet met with great success; but he finds that, except with large differences of temperature or sudden variation in flow, the current is hardly perceptible, and yet without any perceptible current of gas there may be very strong smells perceptible at many ventilators. These would appear to be due to the diffusion of the gases which always takes place, rather than to an actual current: and this will be readily understood by reminding the members of the experiment which shows that if a heavy gas is placed in the lower half of a closed vessel, and a light gas in the upper part, the two parts divided by plaster of Paris, the gases are found to diffuse, even passing through a considerable thickness of plaster of Paris to mix one with the other.

In carrying out this system, the author prefers, where practicable, instead of having the outlet of the upcast shaft at the surface of the road, to conduct a pipe by easy lines up the side of a house or other convenient place. Where this can be done it is not essential in many instances to filter the sewer gas, but where it cannot be done the author has found, after nearly twelve months' experience, that the filter of the form shown by the model effectively does away with any objectionable smell from the sewer gas, the charcoal cannot be displaced to allow the free passage of the gas, and both the form of the filter and the overflow for the water provided in the dirt box prevent the charcoal becoming wet or damp, even after many months' use. The vapor arising from the sewage is condensed on the underside of the filter without wetting the charcoal.

A very similar arrangement to that above described has been used by the author in the drainage of the Town Hall, Edmonton, and there is no difficulty in the general application of the system for house drainage as well as sewerage.

The author is an advocate for trapless valve closets and drains without siphons, and for the straightest possible lines of soil pipes and ventilating pipes, so as to maintain a free circulation from each air inlet to the adjoining gas outlet, and the air ought, in the author's opinion, to have a free circulation through every foot of sewer drain and soil pipe. The variation between the day and night flow in this system secures fresh supplies of atmospheric air in each length of each sewer every night. The system can in most cases be easily adapted to existing arrangements of sewerage and house drainage.

It has now been in work adjoining the Town Hall, Edmonton, for twelve months, and for nearly nine months in another street at Edmonton; and Mr. Laws, the engineer, of Newcastle, who saw the model at the Exhibition this year, where

it was awarded a medal, has been twice to Edmonton to inspect its working, and intends trying it in certain places in Newcastle which have hitherto given trouble.

The author finds that the bottom sections of the valve are not required, and, indeed, the system seems to work best without them. It is not even necessary to stop the back flow of sewer gas that the valve should reach the sewage at its normal level, although the author prefers that it should nearly, if not quite, touch it.

No stoppage or heading up of either of the sewers where it has been used has taken place, although one of them has only a fall of 4 feet in a mile.

In concluding this paper, the author would again call attention to the fact that under this system no lodgment or deposit can take place between the closets in the house and the final outlets, the circulation of both air and water being quite free throughout; and the whole system of sewers is so divided into sections that there can be no accumulation of sewer gas at any one point, and any gas generated is well diluted with air, and well filtered before it escapes into the atmosphere.

The temperature in the sewers is lower than under any other system. Any kind of filter or disinfectant can be used in connection with the system above described, and it has been found satisfactory as regards the prevention of smells both from the street manholes and house drains. The manholes are usually constructed in concrete.

PRIME MERIDIAN TIME.*

From "Nature."

On the first day of the month, the President of the United States, in his message at the opening of Congress, referred to the International Meridian Conference, lately convened in Washington, in the following words: "The Conference concluded its labors on November 1, having, with substantial unanimity, agreed upon the meridian of Greenwich as the starting point whence longitude is to be computed through 180° eastward and westward, and upon the adoption for all purposes for which it may be found convenient of a Universal Day, which shall begin at midnight on the initial meridian, and whose hours shall be counted from zero up to twenty-four."

The Canadian Institute is peculiarly interested in this announcement. No society, literary or scientific, has taken a more important part in the initiation of the movement to reform our time system, of which the success is, to some extent, indicated in the President's words. It therefore appears to me fit and proper that I should recall to your attention the various steps which from time to time have been taken, so that we may possess a record of the events which have led to the now almost general recognition of the necessity for a new notation.

Six years ago, on several occasions, the meetings of the Institute were engaged in discussing the subject of time-reckoning, and the selection of a prime meridian common to all nations. Papers were read and arguments wese advanced, with the view of showing the necessity of establishing a cosmopolitan or universal time, by which the events of history might be more accurately recorded, and which would respond to the more precise demands of science, and generally satisfy the requirements of modern civilization. The "Proceedings" of the Institute for for January and February, 1879, give at considerable length the views submitted and the suggestions offered to meet the new conditions of life. While, on the one hand, it was argued that the introduction of a comprehensive scheme by which time could be universally reckoned was highly desirable, it was equally maintained that the determination of a common prime meridian for the world was the key to its success, and that the estab-

* This paper, giving the early history of a movement which is now attracting such general attention, was reprinted in *Nature* from the "Transactions" of the Canadian Institute.

lishment of such a meridian, as a zero, recognized by all nations, was the first important step demanded.

These "Proceedings" were brought under the notice of His Excellency the Marquis of Lorne, then Governor-General of Canada. In the name of the Institute, they were submitted, in the form of a memorial, with the hope that His Excellency would see fit to lay them before the Imperial Government, that they would by these means obtain the attention of the several scientific bodies throughout Europe, and that some general systematic effort would be made in the right direction to secure the important objects sought to be obtained.

Through the good offices of His Excellency, copies of the Canadian Institute "Proceedings" found their way to the British Admiralty, the Astronomer Royal, Greenwich, the Astronomer Royal for Scotland, Edinburgh, the Royal Society, the Royal Geographical Society, the Royal Astronomical Society, the Royal United Service Institute, and other societies of eminence and weight in the United Kingdom. Copies of the papers were likewise sent through the Imperial Government to the Governments of the the following countries viz.:

France,
Italy,
The United States,
Austria,
Brazil,
Japan,
Spain,
Switzerland,
Greece,
Germany,
Norway and Sweden,
Russia,
Belgium,
Denmark,
The Netherlands,
Portugal,
Turkey,
China.

In the year following, the American Metrological Society issued a report of the Committee on Standard Time. The report bears the name of Mr. Cleveland Abbe, the chairman of the committee, and the date of May, 1879. It draws attention to many of the causes calling for the establishment of accurate time, and the attempts made since the establishment of the electro-magnetic telegraph to make the notation of time synchronous. While pointing out that this result had been obtained in Great Britain through the efforts of Professor Airy, Mr. Cleveland Abbe gave a list of the various observatories on this continent, which are in possession of the necessary apparatus and force proper to furnish astronomically accurate time by telegraph. Writing in February, 1880, while giving the resolution adopted by the Society, recommending the adoption of accurate time by telegraph, from an established astronomical observatory, Mr. Cleveland Abbe points out that the subject of accurate time has been taken up by the Horological Bureau of the Winchester Observatory of Yale College, and that the most perfect apparatus had been received for the purpose of distributing New York time with the highest degree of uniformity and accuracy.

Mr. Cleveland Abbe's own remarks on the subject are of high value. He forcibly points out the difficulties and inconveniences under which railway operations in America labor from the want of a proper system of time. To show this fact in greater force, he gives the 74 standards then followed. These several standards he proposed to set aside and replace by standards each differing one hour, or 15° of longitude.

While recommending this course, the report sets forth that the change would only be regarded as a step towards the absolute uniformity of all time-pieces, and the Society passed resolutions, that absolute uniformity of time is desirable; that the meridian six hours west of Greenwich should be adopted as the National Standard to be used in common on all railways and telegraphs, to be known as "Railroad and Telegraph Time": that after July 4, 1880, such uniform Standard Time should be the legal standard for the whole country, and that the State and National Legislatures should be memorialized on the subject.

Mr. Cleveland Abbe, in this report alluded to the previous "Proceedings" of the Canadian Institute.

The active sympathy of the Marquis of Lorne greatly aided the movement of time reform in its early stages. In 1879, in his official position as Governor-Gen-

eral, he had been the recipient of the papers published by the Canadian Institute, and had transmitted them to Great Britain, and, through the Imperial Government, to the several European centers. In 1880, it was learned that the Report to the American Metrological Society, above alluded to, would shortly be issued. Accordingly, advance copies were obtained from New York, and, together with additional papers issued by this Institute, they were transmitted by His Excellency to the following European Societies, and the special attention of their members was directed to the documents themselves:

1.	The Institut de France...	Paris.
2.	Société de Géographie....	Paris.
3.	Société Belge de Géoraphie	Brussels.
4.	Königliche Preussische Akademie der Wissenschaften....	Berlin.
5.	Gesellschaft fur Erdkunde	Berlin.
6.	Kaiserlische Akademie der Wissenschaften ...	Vienna.
7.	K. K. Geographische Gesellschaft.....	Vienna.
8.	Nicolaievskaia Glavnaia Observatoria	Pulowa.
9.	Imper. Rousskae Geografi-cheskoe Obschestou....	St. Petersburg.
10.	Imper. Akademia Nauk..	St. Petersburg.
11.	Société de Géographie....	Geneva.

By this means attention was obtained for the subject in Europe, and when I submit evidence of the fact, I think you will agree with me that no little of the success which has attended the movement is owing to our late Governor-General. We must all acknowledge how much we are indebted to him for the great personal interest he has always shown on the subject. We are certainly warranted in forming the opinion that the dissemination of these papers, under such distinguished auspices, awakened attention to the arguments they contain. and prepared the way for the subsequent action taken at the International Geographical Congress at Venice, at the Geodetic Congress at Rome, and, more recently, at the Conference at Washington.

Mr. Wilhelm Förster, Director of the Berlin Observatory, enters into the subject at length in a paper "Zur Beurtheilund Einiger Zeitfragen, insbesondere gegen die Einfuhrung einer deutschen Normalzeit." [A review of some considerations on Time, especially against the introduction of German National Uniform Time.]

Mr. Förster proceeds to say: "The British Government is now transmitting, through its representatives, although, at the same time, it declares itself neutral, a proposition which has been published by a society of scientific men in Canada, which aims at the establishment of a cosmopolitan normal datum (Prime Meridian) and of Universal Time, and also the establishment of 24 meridians of an hour apart, by which local time will be absorbed." The first proposal Mr. Förster describes as an important sign of the times, and evidently favors it.

He strongly protests against the establishment of a National German Time; but for railway business, and for such matters of communication as require precision, also for the form of expression of all scientific relations to time, Mr. Förster points out that a Universal Time common to the whole world is to be recommended.

Dr. G. von Boguslavski, in the *Verhandlungen der Gesellschaft fur Erdkunde* ("Transactions of the Geographical Society of Berlin"), commends the new scheme as it has been put forth in the Canadian Institute papers, and foretells that it will be a matter of fact in a short time.

Col. Aden, Director of the Military School, Belgium, has two papers in the *Bulletin de la Societe Belge de Géographie*. He supports the proposal to establish Universal Time, and expresses the opinion that longitude throughout the world should have a common notation, dating from one universally-adopted Prime Meridian.

Col. Wauverman, President of the Geographical Society of Antwerp, in the *Bulletin* of that Society, 1882, advocates the change, and with ability meets the arguments raised against it, showing them to be groundless and arising from a want of thoroughly understanding the question.

In Spain, the proposals have met with full support. All the papers issued by the Canadian Institute have been translated and published in a paper of 80 pages by the Revista General de Marina. The translator, Don Juan Pastorin, an officer of the Spanish navy, is warm in his commendation of the scheme, and

takes a wise and comprehensive view of the whole question. The Spanish Government secured the advantage of this gentleman's services as delegate to the Washington Conference.

M. Otto Struve, the well-known Astronomer and Director of the Imperial Observatory, Pultowa, reports on the papers transmitted by Lord Lorne to the Imperial Academy of Science at St. Petersburg. He gives his adherence to the establishment of Universal Time, based, as suggested, on a Prime Meridian common to the whole globe, and strongly advocates counting the hours in one series up to 24.

In England, the Royal Society considered favorably both the establishment of a Universal Time and the determination of a common Prime Meridian. While the present Astronomer Royal, Mr. Christie, takes a favorable view of the question, his predecessor, Sir G. B. Airy, reported unfavorably. The report of the Astronomer Royal for Scotland, Prof. Piazzi-Smith, is decidedly adverse. These documents have been transmitted to the Institute.

In Italy, the Italian Geographical Society has given its countenance to a work by Mr. Fernando Bosari, who, in a pamphlet of 68 pages, reviews the whole question at length, and lays down three principles: 1. The determination of a Zero-meridian. 2. The establishment of Cosmopolitan Time based upon it. 3. The notation of the hours from 1 to 24 in a continuous series.

The question of a Universal Time and the selection of a Prime Meridian is discussed with ability in a paper published by M. Thury, Professor at the University of Geneva.

At the meeting of the Association for the Reform and Codification of the Laws of Nations, at Cologne, Prussia, in 1881, the question of regulating time on the new system was considered and resolutions moved.

In the same year (1881), the subject occupied the attention of the International Geographical Congress at Venice, at which a delegate from the Canadian Institute attended. The general question was warmly discussed and resolutions adopted. The appointment of an International Conference to meet at Washington, specially to consider the question, was then suggested by the Canadian delegate, and warmly supported by gentlemen representing the Government and scientic societies of the United States. The President of the Congress communicated the resolutions to the Italian Government, and Prince Teano, on behalf of the Italian Government, undertook to conduct the official correspondence. Out of this appears to have sprung the important discussion at the meeting of the International Geodetic Association at Rome, in October 1883, when the utility of Universal Time was recognized, and a special International Conference for the establishment of a zero-meridian for longitude and time recommended.

Returning to this side of the Atlantic, the question of regulating time for railway, telegraph, and civil purposes generally, was considered at the Convention of the American Society of Civil Engineers, held at Montreal, June 15, 1881, and a committee of men engaged in the management, and familiar with the economy of railways, appointed to examine the question. The committee has reported from time to time. They recognized that a proposition to reform the general time system of the country was a problem beset with difficulties, but it did not appear to them insolvable. It was felt, however, that the question affect so many interests that any change could only be effected by general concurrence.

To attain the end proposed by this society, the papers bearing on the question were printed, and a scheme modified on the "Proceedings" of the Canadian Institute was drawn up, under the title of "Cosmopolitan scheme for regulating time."

I may briefly recall the features of the scheme.

There should be one standard of absolute time, a Universal Day, based on the mean solar passage, at one particular meridian, the Prime or initial meridian for computing longitude. This Prime Meridian, together with the Universal Day, to be observed by all civilized nations.

There should be 24 secondary or hour-meridians established, 15 degrees of longitude apart, beginning with the Prime Meridian as zero.

To distinguish the Universal Day from

local days, it should bear the title of "Cosmic Day." *

Cosmic Time is intended to be used to promote exactness in chronology, and to be employed in astronomy, navigation, meteorology, and in synchronous observations throughout the world. To be employed in ocean telegraphy, and generally in all operations non-local in character.

The several 24 meridians to be used as standards for local time around the globe. Applying the system to North America, the effect would be to reduce the standards to four or five, as suggested by the Metrological Society.

A circular, dated March 15, 1882, signed by Mr. John Bogart, the Secretary of the American Society of Civil Engineers, was forwarded to the leading men in railway direction, either as general managers, superintendents, or engineers, and to men of scientific attainments throughout the United States and Canada. The paper thus circulated contained eleven questions, and categorical replies were invited to them.

Replies were received and reported on at a Convention of the Society, held in Washington on May 15, 1882. The scheme submitted was generally and cordially approved.

An emphatic and unanimous opinion was expressed, that there should be established as early as possible a comprehensive system of Standard Time for North America.

Of those who replied to the queries, 95 per cent. favored the idea that there should be a common agreement between the standards of time in all countries. That while we must primarily look to our own convenience on this continent, it is proper to aim at eventually attaining general uniformity among all nations.

Seventy-six per cent. were in favor of reducing the standards in North America so that they would differ only by intervals of one hour, and 92 per cent. were in favor of a notation of the hours of the day by a single series from 1 to 24, instead of in two divisions, each of 12 hours.

The character of the replies received indicated that a remarkable unanimity of opinion prevailed in every section of the continent heard from. The Convention accordingly resolved that an attempt should be made to obtain European concurrence to the selection of a Prime Meridian on which a time-system could be definitely based. But, failing to obtain this recognition, the people of the Western Continent should determine a zero-meridian for their own use and guidance.

It was thereupon resolved to petition the Congress of the United States to take the matter into consideration. The American Metrological Society about the same time adopted a similar proceeding. The consequences were that a joint resolution of the House of Representatives and the Senate was passed, authorizing the President of the United States to call an International Conference to fix on and recommend for universal adoption a common Prime Meridian to be used in the reckoning of longitude and in the regulation of time throughout the world.

On the meeting of the American Association for the Advancement of Science in Montreal, in July, 1882, the subject was brought forward, and all the documents were submitted and discussed. It was agreed that the Association should co-operate with other bodies in furtherance of the movement.

On two occasions the Royal Society of Canada has had its attention directed to the matter, and this body has assisted in furthering the determination of the problem by its co-operation and by correspondence with the Government.

While some delay took place in summoning the International Conference by the President, in consequence of diplomatic correspondence on the subject, the question was ripening on both sides of the Atlantic for concerted action. Indeed, a decision with respect to the regulation of local time was anticipated by the railway authorities in North America, who adopted the system of hour-standards which had been prominently brought forward as described.

On November 18th of last year (1883), the new system of regulating railway time on this continent came into opera-

* [*Note*.—I may remark that the designation, "Cosmic," was first suggested, independently, by two Canadian gentlemen widely separated, by Mr. R. G Haliburton, then in Algiers, and by Mr. Thomas Hector, of Ottawa. The etymology commends the use of the word. It has been accepted by a number of societies and by many individuals as appropriate and applicable.]

tion. There had been several preliminary meetings of railway managers; the last meeting was a Convention held in Chicago the previous October, and it was then determined immediately to carry out the change.

Mr. W. F. Allen, the secretary of this Convention, who also took a prominent part in effecting the adoption of the change, has given a history of the events leading to it. Upon this gentleman mainly fell the labor of arranging details, and he executed the difficult duties assigned to him with consummate ability. In the words of the historian, the transition from the old to the new system "was put into effect without any appreciable jar, and without a single accident occurring." According to this authority the first newspaper to advocate some change was the *Railroad Gazette* for April 2d, 1870, and it is claimed that as early as 1869 Professor Charles F. Dowd, Principal of Temple Grove Ladies' Seminary, Saratoga Springs, proposed a system of meridians based on the meridian of Washington at intervals of one hour, by which railways should be operated, and that an expression of his views was placed in the hands of the President of the New York and Canada Railroad. The proposition appears to have attracted attention in the "Travelers' Official Guide" of 1872. In 1873 it was brought before the Railway Association of America, not now in existence. A committee was appointed to examine into its merits; they failed to recognize the necessity, and recommended that the question of National Standard Time for use on Railways be deferred till it more clearly appeared that the public interest called for it.

Mr. Dowd's efforts to introduce a National Standard Time to meet the difficulties which were being developed were at the time imperfectly appreciated. He, however, had the satisfaction of seeing a scheme unanimously accepted, and put in operation, which in essential features does not materially differ from that which he advocated; and he himself attended at the meeting of the American Metrological Society, and took part in the proceedings when the details of the new time arrangements were officially narrated.

Prominent among those who have earnestly labored to advance the movement of time reform is the distinguished President of Columbia College, New York. Dr. Barnard has from the first taken the deepest interest in the question, and few men have done so much to bring it to a practical issue. In the "Proceedings" of the American Metrological Society for 1881 will be found a paper prepared by Dr. Barnard in 1872, and presented to an association which has since assumed an international character, and is known as the Association for the Reform and Codification of the Laws of Nations. In this paper Dr. Barnard recommends the selection of Greenwich as the Prime Meridian for the world, and he submits the views he held at that early date, which at this hour are of peculiar interest. He points out that "it is becoming a matter of greater importance every day that there should be established some universal rule for defining the calendar day for all the world."

I have alluded to the valuable report of Professor Cleveland Abbe, of the United States Signal Service, to the Metrological Society, and I cannot deny myself the pleasure of acknowledging the services of the gentlemen with whom I have been associated on the special committee on Standard Time of the American Society of Civil Engineers, Mr. Charles Paine, of New York; Mr. Theodore N. Ely, of Altoona, Pennsylvania; Mr. J. M. Toucey, of the Hudson River Railway; Professor Hilgard, Coast Survey, Washington; Professor T. Egleston, of Columbia College; General T. G. Ellis, of Hartford, now unfortunately deceased, and Mr. John Bogart, Secretary of the Society.

The American Society of Civil Engineers, since meeting in Montreal, in 1881, has made persistent and continuous efforts in the common interest to advance the movement of time reform, having greatly aided in bringing about the important change carried into effect a year ago. This Society is now directing attention to a reform of scarcely less importance, the notation of the hours of the day. At the Buffalo Convention in June, 1884, this particular question received prominent consideration in the address of the President, as well as in the report of the special committee. Since that date a correspondence has taken place between the Secretary and the railway managers

in the United States and Canada. Already replies have been received from the representatives of some 60,000 miles of railway, 98 per cent. of whom have given expression to their sympathy with the movement, to abandon the old practice of halving the day, designating the two sets of twelve hours by the abbreviations A. M. and P. M., and are prepared to adopt a simple notation of 1 to 24 in a single series. The great telegraph interests of the country are likewise in full sympathy with it. The President of the Western Union Telegraph Company, Dr. Norvin Green, states that their telegraphic traffic is equal to the transmission of 44,000,000 messages a year, and the general adoption of the 24 o'clock system (as it has been designated), would be cordially welcomed by telegraphers. It would reduce materially the risk of errors, and to the company over which he presides, he says it would save the transmission by telegraph of at least 150,000,000 letters annually.

The branch literature bearing on the two questions of Universal Time and the establishment of a Prime Meridian, has been enriched by a series of papers which have appeared during the past year in the *International Standard*, a magazine published in Cleveland, Ohio. These papers are by the following gentlemen, connected with the International Institute: Rev. H. G. Wood, of Sharon, Pennsylvania; Professor C. Piazzi-Smith, Astronomer Royal for Scotland; Professor John N. Stockwell, Astronomer, Cleveland; Mr. Jacob M. Clark, C. E., New York; Mr. William H. Searle, Pennsylvania; the late Abbe F. Moigno, Canon of St. Denis, Paris; Commodore Wm. B. Whiting, United States Navy; Mr. Charles Latimer, C. E., Cleveland, and others.

It will be seen from what I have submitted, that the proceedings have neither been few nor without success, and that since this Institute published the first issue of papers on Time and Time-reckoning, the subject has received much attention on both sides of the Atlantic. Societies with kindred pursuits, men of recognized merit in the scientific world, have turned to its examination and aided in its development. Some few men have acted in concert. The labors of others have been independent. Some of these names I have been able to record, but I fear that I neglect to include many of eminence because they are not known to me. It is this varied and widely diffused effort which has rendered possible the realization of the practical results which I have the gratification to record, and all the members of this Society must equally join in the common satisfaction in the measure of success which has been achieved.

Six years back, when the subject was discussed in this hall, there were probably not a few who viewed the propositions then submitted as merely fanciful theories. Others, who did not refuse to recognize their bearing, entertained the feeling that many grave difficulties presented themselves to interfere with any successful attempt to reform or modify usages so ancient as the computation of time. But the Institute, as a body, was hopeful. The action taken by the Council to extend the field of discussion and awaken the attention of foreign communities, evinced confidence, and we may now ask, was this confidence justified? What are the facts to-day? Twelve months have passed since an important change in the notation of railway time was made with general approval throughout the length and breadth of North America; a revolution in the usages of 60,000,000 of people has been silently effected and with scarcely a trace that it has happened. That proceeding has been followed by events of equal importance. On October 1st last, a body of accredited delegates from the different nations, on the invitation of the President of the United States, met in conference to consider the problem first submitted to the world by this Institute. The delegates were the representatives of 25 civilized nations. The Conference continued during the whole month of October, and, as a body, they came to conclusions affecting all peoples living under our theories of civilization.

It was early understood that a determination with respect to Universal Time was not possible without the general recognition of a Prime Meridian. Hence the importance attached to its choice, that it should be universally accepted.

It is stated that trials made of Natal coal on the local railway have ended satisfactorily.

THE ANTWERP TRAMWAY LOCOMOTIVE TRIALS.

From "The Engineer."

AFTER a trial of four months with various kinds of mechanical motors, carried out with the appearance of conclusive exactness, the Antwerp jury have arrived at a decision concerning their award. This was mentioned by us last week without comment. An interesting paper on the results of the experiments was read before the Society of Arts, on the 20th inst., by Captain Douglas Galton, and this, while serving to demonstrate the satisfactory nature of the trial, in some respects, showed, on the other hand, what we may venture to call very insufficient evidence for arriving at a conclusion and making an award. The various systems of tramway haulage were insufficiently represented, and the arrangements for ascertaining the cost of working by electricity were very incomplete. The experiments lasted about four months. Five competitive systems offered themselves, which may be classed as follows: Three were propelled by the direct action of steam, and two were propelled by stored-up work supplied from fixed engines. The former were: (1) The Krauss locomotive engine, separate from the carriage. (2) The Wilkinson locomotive engine, *i. e.*, Black and Hawthorn —also separate from the carriage. (3) The Rowan engine and carriage combined. The second class, or those propelled by stored-up force, were: (4) The Beaumont compressed air car. (5) The electric car worked by a secondary battery of 2,464 lbs. weight. Captain Galton very mildly comments on the inadequacy of the range of the experiments when he says it is to be regretted in the public interest that other forms of mechanical motors, such as the Mekarski compressed-air engine, or the engine worked with superheated water, or cable tramways, or electrical tramways, were not also present for competition.

The engines named above include three out of the five known systems of tramcar haulage, only three being represented at Antwerp. The jury could not, of course, alter this or compel makers to attend or go to the expense of building a cable tramway. They might, however, have taken into consideration the fact that only half of the best systems of working tramways by mechanical power were represented at Antwerp. The experiments were made upon a line of tramway laid down for the purpose from near the main entrance of the Exhibition to the principal railway station, a distance of 2,292 meters, or 1.4 mile, beside which there was a triangle for turning of 500 meters, or 0.31 mile. Out of the whole length of the line, viz., 2,797 meters, 2,295 meters were in a straight line, 189 meters in curves of $1\frac{3}{4}$ chain radius, and 313 meters in curves of 1 chain radius. There were on the line four passing places, beside a passing place at the terminus; these were joined to the main line by curves of $1\frac{3}{4}$ chain radius. The line was practically level, the steepest incline being 1 in 1,000, so that the experiments cannot be said to have tested the capabilities of the motors for dealing with lines on which horse traction becomes most expensive.

A regular service was established according to a fixed time table. Each journey was reckoned as starting from the end near the Exhibition, proceeding to the beginning of the triangle, and returning to the starting point. An hour was allowed between the commencement of each journey, fourteen minutes were allowed for a stoppage at the end near the Exhibition, and eighteen minutes at the other end—thus allowing twenty-eight minutes for traveling 2 miles 1,500 yards, or a traveling speed of about six miles an hour. The motors were require to work four days out of six, and on one of the four days to draw a supplementary carriage. The conditions and circumstances were thus of the easiest and in every respect of the most favorable kind. As an explanation of the fact that the cars ran only four days per week, it was explained that there was not room in the service for the cars to work more than this proportion of the whole time. It will thus be seen practically unlimited time was available for

nursing any car or motor that needed it, and it is quite certain that the wealthiest tramway company could not afford to have as much plant standing idle as this represents.

A large number of conditions were laid down as requirements, but we need not refer to them here. We need only refer to those which tramway companies would have most to consider, and these will be gathered from the figures we will give from Captain Galton's paper, premising that all the cars satisfied the regulations concerning the emission of steam and smoke, brake power and appearance. The dead weights per paying load were: Electric car, 1.78; Rowan, 2.3; compressed-air, 2.55. In this comparison it is necessary to remember that both the Rowan and the Beaumont car possessed much greater power and range of power than the electric, both being made to deal with considerable gradients and heavy traffic.

The electric car stands first in respect of rapidity with which it can be brought into action from the shed, as it can receive its battery more rapidly than even the boiler for heating the compressed air could be got ready for use. The Rowan engine stands next, obtaining steam of 8 atmospheres in forty minutes, the Wilkinson requiring forty-seven minutes. In consumption of fuel per train mile, the Rowan stands first, using 5.42 lbs.; electric (estimated) 6.16; Wilkinson, 8.82; Krauss, 9.1; and the Beaumont, 39.48. The Rowan is, however, far ahead of the others in consumption of fuel per place indicated, per mile, using but 0.09 lb. per mile, the electric and the Wilkinson used 0.18, the Krauss 0.20, and the Beaumont 0.69; and the Rowan is still further ahead of the others in fuel per seat per mile run, allowing 16 in. per seat, using but 0.10, while the electric and the Wilkinson used 0.23, the Krauss 0.25, and the Beaumont 0.66. The Rowan and the electric were the same in consumption of oil and tallow, namely, 0.038 lb. per mile, the others being all much higher. The Rowan, having a condenser, was not only the most economical in fuel, but used only 0.75 gallon of water per mile, as compared with 1.06 by the Beamont compressed air, 5.89 by the Wilkinson, and 6.52 by the Krauss.

The consumption of fuel for the electric car cannot be taken as at all accurate, "because the engine which furnished the electricity to the motor also supplied electricity for electric lights, as well as for an experimental electric motor which was running on the lines of tramway, but was not brought into competition." Anyone who has had any experience of electricians' estimates of power used by them, will know that the above facts pretty considerably complicate the estimate, and go far to vitiate its value. The conclusion arrived at by Captain Douglas Galton, who was the English juror, is that the electric is undoubtedly the preferable tramway motor if it can be relied upon, and if the accumulators and machinery can be made durable in the service; and assuming this durability and trustworthiness, then the Rowan engine is the best under every consideration. In spite, however, of the unproved practicability in long and severe service, the jury have determined, we believe, to award the electric car the first prize and to give the Rowan a gold medal, a decision which does not seem to be supported by the results either of the trials or experience. There is no doubt that the electric car is very highly deserving of commendation; but why it has been placed first in the list of awards, except that it promises to suit Antwerp traffic very well, does not appear. The award has no value whatever in England, for it was arrived at without sufficient tests; and by comparison of only three out of six of the systems now in use; and if we consider the cable system and the electric system as now in operation at Blackpool, we must think that two of the most promising of all methods of tramway working were not included in the trials.

A NEWSPAPER correspondent describing the American watch manufactory at Waltham, Mass., and in speaking of the astonishing minuteness of some very essential parts of the watch, says:—"A small heap of grain was shown to us, looking like iron filings or grains of pepper from a pepper castor—apparently the mere dust of the machine which turned them out—and these, when examined with a microscope, were found to be perfect screws, each to be driven to its place with a screwdriver. It is one of the statistics at Waltham worth remembering that a single pound of steel, costing but 50 cents, is thus manufactured into 100,000 screws, which are worth 11 dollars."—*The Engineer*.

THE RELATIVE MERITS OF ELECTRICITY, GAS, AND OIL AS LIGHTHOUSE ILLUMINANTS.

From "Nature."

THE Committee appointed by the Trinity House to report on the merits of electricity, gas and mineral oil as lighthouse illuminants have recently issued a valuable report giving an account of the investigations carried out under their directions, and the conclusions they have arrived at. The committee consisted of Elder Brethren of the Trinity House. They were assisted by Mr. A. Vernon Harcourt, who was appointed by the Board of Trade to co-operate with the Committee, and by Professor W. Grylls Adams and Mr. Harold Dixon, in the more purely scientific part of their investigation.

Three temporary lighthouses were erected on South Foreland, and fitted up for electricity, gas and mineral oil; the optical arrangements were "multiform" in all three—that is, consisted of several similar sources of light, each with its own condensing lenses, superposed; in the case of the electrical tower there were three superposed lamps, as was also the case with the oil tower; but in the gas tower there were four lamps; the two former were therefore "triform," whereas the latter was a "quadriform" light. Any one lamp in either tower could be lighted independently of the others, so as, for instance, to perform biform electricity to be compared with triform oil and quadriform gas.

The lamps for the electric light, and the magneto-electric machines for working them, were supplied by M. de Meritens; the gas apparatus was that of Mr. Wigham, each burner consisting of 108 jets in concentric rings, of which a part only might be employed; the oil lamps in the third tower during the greater part of the trials were six-wicked Douglass pattern, but burners of this description with seven and eight concentric wicks were also tried at various times during the progress of the experiments.

In addition to the temporary lighthouses, three observing huts and a photometric gallery 380 ft. long were erected.

The actual observations that were made may be divided into two classes—eye estimations, and photometric measurements. The former were made by the Elder Brethren, by officers on board the light vessels in the neighborhood, by merchant officers in passing ships, and by the coastguard officers at those stations from which the lights were visible. These eye observations were of two kinds—(1) Estimations of the comparative brilliancy of the lights; (2) definite statements as to the various distances at which the lights were visible in hazy or foggy weather.

With reference to observations of the first kind, they were conducted in accordance with regulations issued by the Trinity House Committee; the observers were instructed in filling in the books of forms which were issued to them, to put down in one column the light from the electrical tower as 100 and in the other column the estimated brilliancy of the lights exhibited by the other two towers as compared with it. It seems probable that the recorded numerical values of the relative brilliancy of the lights can only be a very rough approximation, and that the figures can hardly be taken as indicating with any degree of precision how much brighter one or other of the lights was on any particular occasion. This would probably be admitted by all who have any acquaintance with actual photometric measurements, and who therefore know how difficult it is to form any reliable judgment of the relative illumination of two surfaces, even when these surfaces are actually in contact, excepting the relation of equality. In the case of the experimental lights the comparison must have been rendered still more difficult by the fact that what was to be compared was not the comparative illumination of two moderately bright surfaces in close proximity, but the comparative brilliancy of two lights at some distance from each other, their very brightness adding to the difficulty.

Still, these estimations are manifestly

valuable as setting forth in a clear and unmistakable form that, to the average observer, a particular light appeared the most brilliant; and such seems to have been the way in which they were regarded by the Committee, for on page 21 they state "it will be evident that by mere eye-measurement proportions can only be approximately determined, although the order of superiority may be accepted as proved."

The results of these determinations are set forth in four tables, from which it appears that in clear weather, and in weather that, although not absolutely clear, was not very foggy, there was no question as to the absolute superiority of the electric light over both its competitors, the electric light in the single form having a superiority of more than 30 per cent. assigned to it, as over gas, or oil, in their highest powers (*i. e.*, quadriform for gas, and, triform for oil); the large-sized gas-burner, with 108 jets, appears to have been slightly superior to the six-wick oil-burner, and, consequently, the quadriform gas to the triform oil.

The eye observations of the second kind, those in which the distances at which the lights were visible in foggy weather were recorded, gave much the same result; that the electric light penetrated through the fog to the greatest distance, and that the oil and the gas were about equal in their penetrating power.

These observations also showed that in the case of the electricity the best result was obtained when the currents produced by two or even three machines were sent through a single lamp, and not when each of the lamps was worked by its own special current.

The photometric measurements were carried out by Mr. Dixon, Mr. Harcourt's pentane flame being used as the standard. As is well known, Mr. Harcourt's standard is an air-gas flame which, unlike the so-called standard candles still commonly used for photometric purposes, is not subject to irregular variations in its light-producing powers. Part II. of the Report contains a full account of the standard flame, and the two arrangements for producing it, both of which were in use at the South Foreland. In Mr. Harcourt's original arrangement the air-gas was made and stored in a gas-holder by causing a volume of pentane to diffuse into a known volume of air, and then burning the mixture under certain definite conditions which could be accurately produced at all times. The conditions were such that the flame emitted the same amount of light as an average sperm candle burning under the conditions laid down in the Acts of Parliament which control the quality of the metropolitan gas supply, an amount of light which may differ considerably from that emitted by any single candle.

Mr. Harcourt's pentane lamp was also used; in this arrangement the air-gas is produced as it is required. The lamp is very simple in construction, and the flame is just as constant as in the older form, and as easily regulated, whilst, unlike the older form, the lamp is extremely portable, the whole apparatus not occupying much more space than a packet of candles.

Two kinds of photometer were used—a bar photometer with a Leeson star disk, and Mr. Harcourt's table photometer. The latter is a variety of shadow photometer, and possesses two special advantages—(1) In common with all shadow photometers the two sources of light are on the same side of the illuminated surface, and therefore there is less risk of the results being rendered untrustworthy by diffused or accidentally reflected light than when, as in the more commonly employed arrangements, the sources of light are on opposite sides. (2) The comparison being made by altering the size of the flames, and not their distance, the two portions of the illuminated surface do not alter their relative position, and are always in that which is most favorable for comparison, accurate juxtaposition. The difference in color between the arc light and the pentane rendered it impossible to employ the shadow photometer for the estimation of the electric light. For these measurements a Leeson star disk was employed, and it was found that reliable measurements could be obtained by placing the disk between the two lights and moving it to and fro until a pattern of the star was equally distinct on either side, although on the two sides the colors of the pattern and the background were reversed.

There was so little difference between

the color of the gas and oil flames, and that of the pentane flame, that in the case of these two illuminants measurements could be made both with the star disk and with the shadow photometer.

Comparisons were made in the photometric shed of the light emitted by the De Meritens electric lamp; the Wigham gas burners with different numbers of jets up to the maximum of 108, the Douglass Argand gas-burner, the Sugg gas-burner, and the Siemens regenerative gas-burner, and also the six and seven-wick Douglass oil-burners.

The amount of light emitted by each of the experimental lighthouses was also determined, the observations being made in the huts which had been erected for this purpose at different distances from the towers. At the hut nearest to the towers, the light from all the burners could be compared directly with the pentane lamp giving the light of one candle, but at the second hut only the electric light and the higher powers of the gas and oil lights could be directly compared with the pentane lamp; the single gas and oil lights had to be condensed by a lens before accurate measurements of them could be taken; an achromatic lens, lent by the Astronomer Royal, was used for this purpose. The fraction of the light lost by the absorption and reflection of the lens was experimentally determined and allowed for in the observation.

The general result of a very large number of observations appears to have been that there is but little to choose between oil and gas, as far as their illuminating powers are concerned, and that electricity is greatly superior to both.

The experiments brought out one fact of great practical, as well as scientific interest—that remarkable changes in the transparency of the air occur without any visible haze or mist. To quote Mr. Dixon's words—"Invisible clouds seemed to float by, obscuring the lights for a time as they passed across our line of vision. Sometimes the French lights at Calais and Cape Grisnez showed brilliantly, when the photometer at Hut 2 proved that the lights from the experimental towers, only a mile and a quarter away, had lost one-fourth to one-third their power."

With a view of further investigating the fog-penetrating powers of these different lights, the photometer shed was filled with an artificial steam fog, by means of a pipe brought from the boiler of the engine-house, and the 108-jet Wigham gas-burner, and an electric arc fed by one machine, were pitted against each other, and the distances from which the lights could be seen determined. In all cases the electric arc became visible before the gas flame, as the observers walked up the shed towards the lights, confirming the other eye-observations which have been already mentioned.

The experiments showed also that the electric light suffered a greater proportional loss than either of the two other illuminants when passing through fog or haze, but that, owing to its far greater initial intensity, it nevertheless exceeded the other lights in its penetrating power.

The Committee add to their report some account of the cost of the three illuminants, from which it appears that there is but little difference in the first cost of the electric and gas systems, the latter being slightly the more costly; but, on the other hand, the annual cost of the gas is estimated at rather less than that of the electricity. The cost of the mineral oil apparatus is estimated, both for its installation and for its annual maintenance, at about two-thirds that of either gas or electricity.

The general conclusions arrived at by the Committee—conclusions which seem fully borne out by the evidence set forth in the report—are, that the "electric light, as exhibited in the experimental tower at South Foreland, has proved to be the most powerful light under all conditions of weather, and to have the greatest penetrative power in fog"; that for all practical purposes the gas and oil were equal; and "that for the ordinary necessities of lighthouse illumination, mineral oil is the most suitable and economical illuminant, and that for salient headlands, important landfalls, and places where a very powerful light is required, electricity offers the greatest advantages."

GERMAN STEEL RAILS.—The Rhine Steel Works Company has obtained an order for 1,500 tons of steel rails for Spain. The same company and the Bochum Steel Works Company have secured orders for steel rails for the Swedish State lines, 2,000 tons and 4,500 tons respectively.

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS—ANNUAL MEETING JANUARY 20th–21st.—The Annual Meeting of Society was called to order at 10 o'clock, Wednesday, January 20th, President Frederic Graff in the chair. Messrs. Charles H. Swan, J F. Flagg, Robert Cartwright, and F. C. Prindle were appointed tellers to canvass the vote for officers of the Society.

The Report of the Board of Direction for the year ending December 31st, 1885, was presented. There has been during the year an addition of 74 to the various grades of membership in the Society, viz., 56 members, 2 associates, 15 juniors, and 1 fellow. Three juniors were transferred to members. There have been 12 deaths, 5 resignations, and 4 have been dropped from the rolls. Reference was made in the report to the successful convention held at Deer Park, Md., June, 1885; also to the fact that the attendance at other meetings of the Society during the year has been larger than ever before, and that very many non-resident members have been present at such meetings. The collations and informal receptions held after the meetings on the first of each month have provided opportunities for intercourse between members and visiting engineers which would not otherwise have occurred. The collations have been furnished by subscriptions from resident members. No funds of the Society have been used for this purpose.

The "Transactions and Proceedings" for 1885 contain some 160 pages more than have ever been issued in any previous year. The Board devotes the larger part of the income of the Society to the preparation and issue of these publications.

There have been but two contributions to the building fund during the year. Every contribution to this fund reduces the rental paid by the Society for its house, which rental is now in the form of interest on the mortgage and taxes on the property. The house itself is an excellent investment, steadily increasing in value. The Board calls the attention of members to the desirability of reducing the mortgage upon the property by contributions to the building fund from persons interested in the welfare of the Society. During the year but one fellow was elected. The Board refers to the fact that association with the Society as a fellow secures all the publications and a participation in all the privileges, except the right to vote, and that the suggestion of such association with the Society, to many gentlemen not otherwise connected with it, might result in a large increase of this important fund, the income from which is devoted to the publications. The fellowship subscription is, at present, $150 for life.

The only amendment to the Constitution passed during the year was one providing for the compounding for future annual dues by a single payment.

The Board of Censors to award the Norman Medal for the past year, reports the award of that medal to paper No. 300, "Record of Tests of Cement made for the Boston Main Drainage Works, 1878–1884," by Elliot C. Clarke, M. Am. Soc. C. E. The committee to award the Rowland Prize for the past year report the award of that prize to paper No. 295, "Experiments with New Apparatus at Low Velocities," by A. M. Wellington, M. Am. Soc. C. E.

The committee appointed to confer with committees appointed by kindred societies for the purpose of devising and considering a plan for creating a library for the joint use of the organizations represented, reported that meetings had been held with committees from the American Institute of Mining Engineers, the American Society of Mechanical Engineers, and the American Institute of Electrical Engineers; that the members of the joint committee are practically in accord as to the desirability of the joint library and as to the general plan to be adopted. The committee is encouraged with the prospect of success in securing a plan capable of execution, but is not yet prepared to report in detail. The report was accepted and the committee continued.

The committee on Uniform Standard Time reported that practical progress was being made in the direction of notation of the hours from one to twenty-four, and the abandonment of the division of the day into half days; that many important railways were ready to co-operate in this direction, and that it had been publicly announced that the Canadian Pacific Railway was prepared to make this change at an early day. The report was accepted, and the committee continued.

The committee on the Compressive Strength of Cements, and the Compression of Cements and the Settlement of Masonry, reported the organization of the committee and the commencement of investigation of the subject. The committee was continued

A report from a minority of the committee to Consider changes in the Organization of the Society was presented. No report from the majority was presented. The whole subject was referred back to the committee.

The tellers appointed to canvass the ballot for officers of the Society reported the result of the canvass, and the following members were declared elected officers of the Society for the ensuing year: President, Henry Flad; Vice-Presidents, Thomas F. Rowland and Thomas C. Keefer; Secretary and Librarian, John Bogart; Treasurer, J. James R. Croes; Directors, G. S. Greene, Jr., William R. Hutton, W. Howard White, Henry G. Morris, and Charles L. Strobel.

FEBRUARY 3d, 1886.—Vice-President Thomas F. Rowland in the chair. The following candidates were elected as members: Edward Wm. Eckert, Massillon, Ohio; James Ellison Mills, Quincy, Ill.; Edward Thomas Wright, Los Angeles, Cal. As associates—George Lord Burrows, Saginaw, Mich.; Palmer Chamberlaine Ricketts, Troy, N. Y.

A paper by T. H McKenzie, M. Am. Soc. C. E., on the "Water-works of Southington, Connecticut," was read. The town of Southington is on the Qununpiac River, in a valley surrounded by hills rising 600 feet above the river. The water-works were constructed by a private corporation in 1883–84. The water is procured from a mountain stream with a drain-

age-way of 2½ square miles, mostly inclined and with regular surface. The rock is granite. The storage reservoir is formed by an earth dam, 4 miles from and 535 feet above the town. The distributing reservoir is built about 3 miles from the town, and 240 feet above it; the water is conveyed from this reservoir by a cast-iron pipe. The gaugings of the stream during 1883 showed an average daily flow of 4,300,000 gallons. The storage reservoir covers an area of 23 acres, has 25 feet depth of water at the dam, and a capacity of 60,000,000 gallons. The earth for the dam was of solid loam, placed in horizontal layers not more than 6 in. in depth, and each layer wet and rolled with a 3-ton grooved iron roller. A 20-in. cast-iron pipe is laid through the dam on a heavy masonry wall for its entire length, with short cut-off walls every 24 feet. At the outlet of the pipe a heavy masonry apron, with wing-walls, is built on the bed rock.

The distributing reservoir is formed by a masonry dam 20 ft. in height, which covers an area of 1½ acres, and contains 3,000,000 gallons.

The main supply-pipe through the dam is 12 in. in diameter, and from thence to the town 10 in.; the general depth is 5½ ft. It is all carefully laid to grade with very few horizontal bends; there are no vertical bends, except at the four summits, where air-cocks are placed. There are blow-offs at each depression of the grade. Thickness and weight of pipe are adjusted to the pressure to each 100 ft. of head. The pipes are coated by immersion in a bath of heated coal-tar pitch, and were tested to a pressure of 300 lbs. per square inch. The distribution pipes, 8, 6, and 4 in. in diameter, are laid to grade, and at all summits house supply pipes are arranged to act as air-vents. The pressure over most of the town ranges from 90 to 100 pounds. Hydrants are set at all corners, and also at other points, so that they are not more than 500 ft. apart. The whole cost of the works was $88,750, which is moderate, as compared with many other similar works. No leaks have appeared, and no fissures of any kind have occurred in the dams, pipes, or fixtures.

ENGINEERS' CLUB OF PHILADELPHIA—RECORD OF REGULAR MEETING, JANUARY 16th, 1886.—Mr. Howard Constable gave an interesting account of the system used in London for the Public Supply of Hydraulic Power, which he has recently had occasion to examine. The company in London was started in 1882, based upon the success achieved by a similar scheme in Hull, which has been in operation some eight years; it, in turn, was the outgrowth of a system which prevails very extensively in many English manufacturing and warehouse concerns—that of employing hydraulic cranes, hoist capstans, and the like, and operating them from one pumping station. This plan was advocated by Mr. Bramah as early as 1802, and during the past forty years Sir William Armstrong has done much towards putting it into practice by the manufacture of hydraulic plant, so that the advantages of the principle are much more thoroughly understood abroad than with us. The general mind in this country looks with some fear upon high pressures, and thinks the system rather novel, not knowing that in our practice of pumping oil to the seaboard, pressing cotton and extracting oil from seeds, we employ hydraulic pressures ranging all the way from 400 to 5,000 lbs., and over, per square inch, whereas the London Supply Co. use only 700 to 800 lbs.

Their plant consists of about 12 miles of cast-iron mains, distributed throughout the busiest parts of London, on both sides of the river, and as far west as Victoria Station. The mains and branches are laid so as to avoid any dead-ends, and with stop-valves so that any length of 1,000 ft. can be isolated. The water used is taken from the Thames and filtered into tanks, from which it is received by the pumps and passed into two accumulators, thence on through the mains. The pumps are of the vertical, three-cylinder, fly-wheel type, compound-condensing, single-acting plungers, directly connected to piston-rod. The starting and stopping of the pumps is done automatically by the accumulators; and in connection with this, the arrangements for letting live steam into the low-pressure cylinder is very ingenious. The boilers are of the Lancashire type, fitted out with automatic fuel-hoppers, stokers, etc., and fuel elevator. The charges for power water are made upon meter register and a sliding scale of prices. The financial success of the company was almost assured from the first.

Prof. T. M. Haupt read a profusely illustrated paper on Harbors, containing data as to depths, etc., and showing that from New York to the Gulf there were only four natural entrances where the depth at mean low water was over 16 ft., while the largest ships draw from 26 to 28½ ft. He discussed the relation of the forces, stated the general principles to be observed in harbor improvements, commented on the unfavorable results obtained by using submerged jetties, and the reasons therefor, and called attention to a few special cases, which were reserved for a more complete discussion subsequently. A model was also shown, exhibiting a portion of the bed of the lower bay of New York entrance, with the peculiar hole at the head of Gedney's Channel, with an explanation of the forces which had maintained it for many years. He also stated that he believed it entirely feasible to so modify the forces as to change the direction of the resultant and throw it into Gedney's Channel. By such a local treatment an excellent entrance could be obtained to the harbor, at a cost very much less than that proposed to be expended for the submerged dykes—five miles long—and other works, estimated at five to six millions. The various features were freely illustrated by U. S. C. & G. S. and other diagrams.

FEBRUARY 6th, 1886.—The Secretary presented, for Mr. A. R. Cruse, a compilation of tests of the efficiency and economy of various gas burners.

The Secretary presented, for Mr. Mataro Crizuka, a paper, illustrated by Japanese map, upon the Railways in Japan. The history of the present railways is given, and the projects for the future. The paper treats of costs per

mile; the bridges, tunnels, shops, etc.; the methods of operation, the profits of the traffic, the native and foreign supervision and labor, and the relations of the Government and private enterprise to the work.

Prof. L. M. Haupt read a second paper on Harbor Studies, discussing the action of tidal currents, and showing their locus as revealed by the mold of the harbor and the divide or crest line of the bars. He believed that local efforts to improve the channel across the New York bar would be most successful by utilizing the resulting ebb's scour at the head of Gedney's Channel by floating devices for concentration and vertical deflection of the currents there existing, and made a few remarks showing how such floating deflectors might be used to regulate the currents of the Mississippi River and so tend to establish a constant regimen.

Mr. J. Foster Crowell read a paper upon the Present Situation of the Interoceanic Canal Question, presenting the subject from a general standpoint. He sketched the history of the various past attempts to establish communication through the American Isthmus, and traced the developments in the different directions of effort, which finally concentrated the problem upon the three projects now before the world, summarizing the progress in each case, and stating the following propositions:

I. That Panama is the only possible site for a sea-level canal, and that such treatment is the only feasible method at that place.

II. The Nicaragua is the only practicable site for a slack-water system (for a canal with locks), and that it is pre-eminently adapted by nature for such a use; that there are no obstacles in an engineering sense, and no physical drawbacks that need deter the undertaking.

III. That the Ship Railway, as a mechanical contrivance, has the endorsement of the best authorities, and may be admitted to be the *ne plus ultra* as a means of taking ships from their natural element and transporting them over the land.

IV. That none of these plans has as yet advanced sufficiently to warrant our considering its completion as beyond doubt.

V. That, as the *additional* sum now asked for by De Lesseps (*even if sufficient*) to complete the Panama Canal, is *greater* than the estimated cost of either Nicaragua Canal or the Ship Railway, it would be economical to abandon the Panama Canal, and the money sunk in it, to date, unless its location and form possess paramount advantages, and we therefore may profitably consider the relative merits of the three lines, without regard to the past, from four standpoints, viz.:

1. Geographical convenience of location.
2. Adaptiveness to all marine requirements, present and future.
3. Political security.
4. Economy of construction and operation.

He then discussed the comparative claims to excellence. In the first consideration, after classifying the several grand divisions of future ocean traffic, and noting especially the needs of the United States, he claimed that, while there was little to choose, in this respect, between Nicaragua and Tehuantepec, either was far superior to Panama.

In the second particular he maintained that, owing to the characteristics of the Panama Canal and the practical impossibility of enlarging it hereafter, excepting at stupendous cost, it could not serve the purposes of the future, although it might, if completed, supply present need. He praised the ingenuity of the plans for the Ship Railway, but emphasized the fact that it will be the *movement of the traffic*, not merely the lifting and supporting of ships in transit, that will test the system, and suggested that even the beautiful application of mechanical force which had been contrived might be powerless to insure the high grade of service which is an absolute necessity. In this connection the general features of the Nicaragua Canal, in its latest form were referred to, and the opinion expressed that even were all difficulties in the way of the Ship Railway eliminated, it could not be superior to the canal in respect of adaptiveness.

In point of political security he claimed that both Tehuantepec and Nicaragua were reasonably free from doubts, with the advantage in favor of the latter, while at Panama no security, for United States interests at least, could be counted on, without the liability of a military expenditure far exceeding the cost of the canal itself.

The matter of comparative cost of construction and operation was discussed generally, and in conclusion the author stated that "this all-important question is still an open one, of which the future needs of our country justify and demand at this time a most searching scrutiny, and, moreover, our interest and the interest of mankind require that before this century closes, the best possible pathway between the Atlantic and the Pacific shall be open to the navies of the world.

The paper was illustrated with maps and diagrams.

ENGINEERING NOTES.

THE CLEANSING AND VENTILATION OF SEWERS.—At a recent meeting of the Metropolitan Board of Works a report was presented by the Sanitary Committee, making the following recommendations:

1. That any old and defective, disused, or partially disused sewers which may remain in any part of the metropolis should be disconnected from the present sewerage system, cleansed, and filled up; and that, where necessary, pipe or other proper sewers should be substituted.

2. That stringent measures be taken by the Board, and by the Vestries and District Boards (*a*) for preventing road-sweepings passing into sewers, (*b*) for preventing the discharge into sewers from manufactories and other places of improper substances, such as chemical refuse or trade filth, or of hot water or steam, so as to be the cause of nuisance.

3. That the most important requirement for keeping sewers in satisfactory condition is a supply of water sufficient in quantity to carry the sewage in suspension through the sewers.

4. That whenever the supply of water in

sewers is insufficient for cleansing the sewers, effectual provision for cleansing such sewers should be adopted by flushing or other means.

5. That one of the most effectual methods of flushing would be by means of water simultaneously discharged into house drains, as such a simultaneous discharge would flush branch sewers, then local sewers, and finally main sewers; that such a method of flushing can be effectually carried out by householders flushing their house drains periodically and simultaneously at stated times; that a great number of householders would probably be willing to cooperate with the authorities in improving the condition of the sewers in their district in this way; and that it is desirable that the several Vestries and District Boards should intimate upon what days and at what hours householders should flush their drains.

6. That provision near the heads of branch sewers, for flushing such sewers from their commencement, is to be desired.

7. That the practice of flushing the courts, alleys, small streets, etc., in poor districts in summer, is to be highly recommended as a desirable method of flushing sewers and improving the sanitary condition of the districts.

8. That next to effectual cleansing, one of the most important safeguards against nuisance and danger to health from sewers, is the dilution of the gases therein by a constant and plentiful supply of fresh air to the sewers by means of effective ventilation.

9. That the ventilation of sewers in the metropolis by means of ventilating shafts leading to gratings in the center of roadways, has been the cause of complaint, owing to the imperfect manner in which the system has been carried out, the ventilators being deficient both in size and number.

10. That the surface ventilators to the recently-constructed sewers have ordinarily been placed at a distance of from 50 to 60 yards apart, with air openings in the gratings equal to 60 square inches, and that the number and size of many of the ventilators on other sewers in the metropolis should be increased.

11. That the amount of ventilation afforded by large special ventilating shafts is in no way commensurate with their cost, and that the adoption of such shafts, with or without fire heat, or the connection of sewers with factory shafts can only be adopted in very exceptional circumstances. Where shafts with fires are used, the sewer gases should be allowed to pass into such shafts over as well as through the fires; otherwise the amount of ventilation afforded will be very much limited.

12. That pipe ventilators of large section, and constructed with bends and without angles, can be used with great advantage in addition to, and not in substitution for, surface ventilators, wherever the consent of owners and occupiers can be obtained to such ventilators being affixed.

13. That the Committee are of opinion that the ventilation of the sewers of the metropolis may be improved by altering the construction of many of the surface ventilators as previously recommended, and by carrying up large pipe ventilators at convenient points wherever practicable.

14. That the use of charcoal or other appliances for deodorizing gases in sewer ventilators is undesirable, as such appliances do much harm by obstructing ventilation, are costly and troublesome, and are quite unsuitable for general use over a large sewerage system.

15. That during the cold and wet seasons the arrangements suggested in the report for providing ventilation will be sufficient, as putrefaction proceeds so slowly at low temperatures, and when the sewage is diluted with large volumes of fresh water, that the foul matters pass from the sewers before material quantities of offensive gases are generated.

16. That during dry and hot weather the foregoing recommendations respecting the flushing and cleansing be carried out as far as practicable, especially in sewers having only a small flow of water.

17. That in addition to these precautions, a system of deodorization during the summer be arranged for the main sewers, on the plan adopted by the Board during the past summer; that local boards be requested to adopt a similar system with local sewers, and to use all their influence to induce householders to employ in their house drains suitable deodorizing agents during periods of high temperature and drought.

18. That manufacturers and others discharging refuse into the sewers be compelled to so treat their waste water (by such process as they may deem fit, but to the Board's satisfaction), that it shall have no greater *deoxidizing* power than average household sewage.

19. That the Committee are not prepared to recommend the Board to exercise the powers given by the 83d section of the Metropolis Management Amendment Act for making by-laws for the guidance, direction, and control of the vestries and district boards, and other persons in relation to the maintenance, cleansing, ventilation, etc., of sewers, until it is shown that the suggestions made in the report have not been voluntarily adopted.—*Building News.*

A WORK of considerable engineering importance, involving the diversion of the Spey was completed on Tuesday, in connection with a new line along the coast of Banff and Elgin. The Great Northern Railway Company of Scotland found it necessary to construct a bridge across the river about two miles from the mouth. The contractors had to construct the bridge at a point which rendered it necessary to divert the course of the river. It was done by opening a new bed to the left of the old one. Into this bed the water burst on Saturday rather unexpectedly. A great body of water still flowed in the old course, and to prevent this it was decided to form a huge dyke. Bags filled with sand were piled high up against the bed, but these were swept away, as the river narrowed, and the water cut a course 10 ft. where previously it had only been 4 ft. deep. The sand bags were then chained together in twenties, and dropped into the water. After continuous labor for a week, the work was completed on Tuesday, and the whole body of water now flows under the new bridge.

IRON AND STEEL NOTES.

From statistics just published it appears that the production of all descriptions of steel in the United States since 1874 has been as follows: 1874, 241,614 net tons; 1875, 436,575 tons; 1876, 597,174 tons; 1877, 637,972 tons; 1878, 819,814 tons; 1879, 1,047,586 tons; 1880, 1,397,015 tons; 1881, 1,778,912 tons; 1882, 1,945,095 tons; 1883, 1,874,359 tons; 1884, 1,736,985. The principal steel manufacturing State of the Union is Pennsylvania, which, of the 1,736,985 tons of steel made last year, produced 1,157,376 tons.

RAILWAY NOTES.

AUTOMATIC BRAKES.—Continental railway companies, like some of those in this country, continue to afford practical illustrations of the advantages of really efficient brakes. Recently, near Courtrai, on the Belgian State Railway, the engine of an express train left the rails, causing the coupling to snap, and the Westinghouse brake to be automatically applied, thus avoiding most serious consequences. Again, at Thalhausen, on the Wurtemburg State Railway, the express train from Berlin to Milan had a very narrow escape indeed from being thrown into the river Neckar, from a cause which was at one time a fruitful source of disaster in this country. To shunt a wagon into a siding, leave the points open, and take off the main-line signals for an express train running at forty-five or fifty miles an hour, certainly affords an excellent chance of calamitous results ensuing, and these were only prevented on this occasion by the driver observing that the points were open and applying the Westinghouse brake, by which means the train was brought to a stand in the siding only two or three yards from the edge of the steep bank to the river. A further illustration comes to hand from France. The Indian mail, when running at forty-five miles per hour, left the line near Aiguelielle, on the Paris, Lyons, and Mediterranean Railway between Modane and Chambery, it is supposed owing to the spreading of the permanent way. Once more the engine separated from the train, and again the Westinghouse brake was applied automatically, the train being brought to a stand without injury to a single passenger. It will be noted that these accidents were all of that sudden emergency type which we have frequently pointed out is now, and will continue to be, the chief feature of our railway accidents. For such cases it is not sufficient simply to provide a continuous brake. It is perfectly clear that had the brake in either of these three instances given, not been powerful, instantaneous and automatic as well as continuous, it could have been of but little use in averting the terrible results which must almost inevitably have followed in every case. Fortunately, although opinions on the brake question differ on the Continent as to some points, there would appear to be an almost universal feeling in favor of automaticity and high pressure, as against simple brakes and those worked by vacuum, thus agreeing with the practice in America and the Colonies. At a recent meeting of the Society of German Mechanical Engineers at Berlin, a paper was read on the automatic vacuum brake, and some of our own railway companies would do well to weigh the remarks of Privy Councillor Stamke, Chief Technical Adviser of the Minister of Prussian railways. In the discussion which took place this influential official stated that it was quite useless to discuss vacuum brakes, since there could no longer be any question that automatic compressed-air brakes alone would be used over the whole Continent. This opinion receives some confirmation from the fact that it has just been decided to replace the vacuum brake by the Westinghouse on the Rome-Naples section of the Italian-Mediterranean Railway.—*Building News.*

ORDNANCE AND NAVAL.

ARMOR-PLATED TURRETS.—The experiments which have just been concluded at Bucharest with the cupola turrets of the future fortifications of the Roumanian capital deserve attention for various reasons, the principal one of which is tnat they involved a trial of strength between the competing German and French systems. They also excited more than usual interest on account of a report to the effect that General Brialmont, the well-known Belgian engineer officer, who is charged with superintending the construction of those fortifications, had declined to undertake that duty unless the German style of turret was adopted. The unexpected results—unexpected because so much was anticipated from the French turrets—seem to indicate that General Brialmont was right. It appears to have been a foregone conclusion with the Roumanian military authorities that the German turret plates, manufactured by the well-known firm of armor-plate makers of H. Gruson, of Buckau, near Magdeburg, would have no chance against the French plates, supplied by the Société de Saint-Chamond (department of the Loire), when submitted to equally severe tests. The results have falsified the expectations or predilections of those most nearly concerned. Before giving those results, it should be stated that it is intended to convert Bucharest into a *place d'armes* of the first rank, by surrounding it with a girdle of fortifications between 37 and 44 miles long, and consisting of eighteen forts of three degrees of strength. The chief point of strength of the principal fort is to be forty revolving ironclad turrets, distributed amongst the forts, and each armed with two 15-centimeter guns. Having regard to the immense strides made by the artillery of the present day, owing to the introduction of chilled shot and rolled iron, as well as of long-range guns, and the progress achieved in the manufacture of iron armor for ships and land batteries, the decision respecting the turret system was made dependent, at the suggestion of General Brialmont, upon the competitive trials which have taken place, and the importance of which was enhanced by the presence not only of the military representatives of the great powers, but of officers of the Dutch, Danish, and Chinese armies. The respective turrets of the

rival manufacturers were erected on the plateau of Cotroceni, near Bucharest, and armed with Krupp and De Bange guns and mortars respectively. It ought to be added that the system adopted in the French turret is that of Major Mougin, of the French engineers, and in the German turret that of Major Schumann, of the German engineers. At the beginning of the trials public opinion was on the side of the French system, and this appeared to be justified by the reports of the first few days' work. According to them, the French turret, notwithstanding its greater weight, revolved much more easily and more quickly than the lighter German turret. That statement was quite correct, and it was also a fact that the Krupp guns of the Schumann-Gruson turret had nine missfires in the first twenty-five rounds of two guns each, whilst the Bange guns in the French turret fired all the fifty rounds without a hitch. During the following days, however, the Krupp guns, provided with friction fuzes, worked as satisfactorily as the De Bange battery, furnished with electric fuzes, if the quick-match was pulled properly. Although during salvo target firing the Schumann turret could only fire forty-one rounds, more hits were obtained from it than from the Mougin turret with fifty rounds. In quick firing, the German turret fired the salvoes in thirteen minutes, whilst the French turret took twenty-one minutes. The battery of the German turret also excelled that of the French turret in firing at an unknown target indicated by hoisting and as quickly dropping a signaling flag. Still more favorable results were registered for the Schumann-Gruson turret system when the two revolving turrets served as targets to a battery of 15-centimeter Krupp hooped guns, fired at a distance of 1,000 meters. In order to strike the French turret thirty times, only forty-seven rounds had to be fired, whilst, to obtain a like number of hits of the German turret, which is provided with a flat dome, eighty-seven rounds had to be discharged. The mantle of the French turret, consisting of rolled iron 40 centimeters thick, has, besides, suffered considerably by the bombardment, a piece of iron weighing 6 cwt., besides several smaller fragments, having been detached from the enormous iron mantle of the Mougin turret. The German turret, with the exception of a small crack, has not been injured. The firing from mortars and the blasting experiments with dynamite carried on against the enormous chilled hoops outside the base of the two turrets having also resulted in a victory for the German turret, no surprise will be felt when it is stated that the commission of experts have pronounced in favor of the turrets on the Schumann-Gruson principle, and that the French turret has been discarded. General Brialmont's views on the subject have thus been fully sustained, notwithstanding that popular opinion was at first adverse to him. It is stated that the work of fortification of the Roumanian capital is to be proceeded with as soon as provision is made for the large funds required for carrying out the stupendous undertaking, the completion of which will convert Bucharest into the most formidable entrenched camp, next to Paris, in existence.—*Morning Post.*

BOOK NOTICES.

PUBLICATIONS RECEIVED.

PAPERS of the Institution of Civil Engineers: No. 1,995.—The Energy of Fuel in Locomotive Engines. By Granville Carlyle Cunningham, M. Inst. C. E.

No. 2,050.—The Karachi Water-works. By James Strachan, M. Inst. C. E.

No. 2,099.—Concrete Building at Simla, India. By Walter Smith, Assoc. M. Inst. C. E.

No. 2,111.—On an Improved Method of Lighting Vessels Under Way at Night. By Bradford Leslie, M. Inst. C. E.

Abstracts of Papers in Foreign Transactions and Periodicals.

The following Professional Papers of the Navy Department. After No. 12 the series has been known and styled "Naval Professional Papers":

1. Astronomy. By Prof. Chaunnet.
2. Tides and Tidal Phenomena. By Henry Mitchell.
3. Lightning Conductors. Translated from the French by Commander R. Aulick, U. S. N.
4. Report of the Circumnavigation Committee of the Royal Society, 1872.
5. The Marine Compass. By Prof. B. F. Greene, U. S. N.
6. Chronometer Rates. By Lt.-Com. C. H. Davis, Jr., U. S. N.
7. Lecture on the Turning Power of Ships. By W. H. White, R. N.
8. Observations for Dip taken on board the U. S. S. *Adams*. By Commander J. A. Howell, U. S. N.
9. Determination of the Length of a Nautical Mile. By Prof. J E. Hilgard, Supt. U. S. Coast Survey.
10. Iron Ships (Papers and Discussions). Reprinted from "Transactions of the Institution of Naval Architects,"
11. Steel for Ship-Building. Reprinted from "Transactions of the Institution of Naval Architects."
12. Screw Propulsion. Reprinted from the "Transactions of the Institution of Naval Architects."
13. Magnetism; Its General Principles and Special Application to Ships and Compasses.
14. Papers and Discussions on Experiments with Steel. Reprinted from various sources.
15. Papers and Discussions on Ships, Guns and Armor. Reprinted from various sources.
16. Papers and Discussions on Engines, Boilers and Torpedo Boats. Reprinted from the "Transactions of the Institution of Naval Architects."
17. The Magnetism of Iron and Steel Ships. An explanation of the various ways in which it affects the Compass. By Lt.-Com. T. A. Lyons, U. S. N.
18. Training of Enlisted Men: Three Papers reprinted from the "Journal of the Royal United Service Institution."

From Relfe Brothers, 6 Charter-House Buildings, London, E. C.:

A New English Grammar; My First French Course; My First French Phrase-Book. By A. Grover, LL. D.

First Principles of Euclid; First Principles

of French History; First Principles of English History; First Principles of English Grammar. By T. S. Taylor.

First Year of Scientific Knowledge. By Paul Bert.

A Latin Book for Beginners. By C. H. Gibson.

English Spelling as it is.

Relfe Brothers' Model Reading Books, Nos. 3, 4, 5, 6.

Cassell's Magazine of Art for March.

STATISTICS OF HYDRAULIC WORKS AND HYDROLOGY OF ENGLAND, CANADA, EGYPT AND INDIA. By LOUIS D'A. JACKSON. London: W. Thacker & Co.

This work is an enlargement of the second part of the "Hydraulic Manual and Statistics" by the same author. The additions are extended, obtained partly by the author's own work and partly from other reports and records.

The regions are described separately and in the order given in the title. In Great Britain, the river basins, canals, storage works, sewage irrigation, irrigated crops and analysis of water are treated in turn.

Of Canada, only river basins, canals, navigations, geology and meterology are presented.

Of Egypt, the hydrology of the Nile, the irrigation, irrigated crops and analysis of the water occupy most of the space.

Of India and Ceylon, the rivers, canals and tanks or storage works are of chief importance. The history of the progress of these works is full of interest.

The work will prove to be a valuable guide to engineers who are charged with similar works in other countries.

The details of the engineering features are regarded as subordinate to the economic and commercial phases of the improvements, so the statistics relate to expenditures for cost and values received.

The book is well printed but is without illustrations of any kind.

THE THEORY OF STRESSES IN GIRDERS AND SIMILAR STRUCTURES, WITH PRACTICAL OBSERVATIONS ON THE STRENGTH AND OTHER PROPERTIES OF MATERIALS. By BINDON B. STONEY, LL. D., F. R. S., Member of the Institution of Civil Engineers. Price, $12.50. New York: D. Van Nostrand.

The new edition of this well-known standard work exhibits several new and important features. The general principles of graphic statics are introduced early in the treatise, and graphic solutions form a conspicuous feature of the book.

Additions have also been made to the subjects of oscillating stresses, working loads, wind pressure, steel, pillars and riveting.

Full advantage has been taken of the results of tests of materials made known since the previous edition was written The total result is an octavo volume of 777 pages, with 143 illustrations and five folding plates.

The following are the headings of the various chapters: Introductory; Flanged Girders with Braced or Thin Continuous Webs; Transverse Stresses; Girders of Various Sections; Braced Girders with Parallel Flanges and Webs, formed of Isosceles Bracing; Girders with Parallel Flanges, connected by Vertical and Diagonal Bracing; Braced Girders with Oblique or Curved Flanges; Deflection; Continuous Girders; Quantity of Material on Braced Girders; Angle of Economy; Torsion; Strength of Hollow Cylinders and Spheres; Tensile Strength of Materials; Crushing Strength of Materials; Pillars; Shearing Stress; Elasticity and Set; Temperature; Web; Flanges; Wind Pressure and Cross Bracing; Cross Girders and Bridge Flooring; Counterbracing; Deflection and Camber; Depth of Girders and Arches; Working Stress and Working Load; Fastenings and Connections; Estimation of Girder Work; Appendix.

DYNAMO-ELECTRIC MACHINERY.—The following letter to the editor of the *Electrician* (American) will appear in the March issue of that journal:

To the Editor of the Electrician:

SIR:—In the second edition of Prof. S. P. Thompson's work upon "Dynamo-Electric Machinery," which has recently appeared, I notice that the author has reprinted the preface which appeared to his first edition. This original preface contained certain statements which, at the time, I passed over in silence, because I thought that the majority of the readers of his book in this country would readily see the injustice of the reflections contained therein. My reasons for thinking so were, that inasmuch as the statements there made were evidently penned in an unfriendly spirit, and as they seemed to give an unjust impression regarding the dealing of American publishers with English authors, they were at the same time accompanied with a positive declaration that the lectures complained of as reprinted by me, were reprinted as well in the *Electrician* (English), the *English Mechanic*, and other technical journals, and were translated into French; and as Professor Thompson did not intimate that he was the recipient of a check from these journals, or from the French publisher, I imagined that Professor Thompson's readers, in this country, at least, would not judge me harshly for not doing what no one else who had used his lectures had done, or which even he (Prof. Thompson) would seem to have expected them to do.

I think, perhaps, now that this second edition of his extended work has appeared, containing this original preface, that a statement of the case may properly be made.

In 1883 there appeared in the *Journal of the Society of Arts* a series of papers, being certain lectures delivered by Professor Thompson. As is the custom with the editor of my Magazine, when any really valuable contributions appear in that journal, he reprinted them, and they may be found in the ENGINEERING MAGAZINE for March, April, and May, 1883, duly credited to the *Journal of the Society of Arts*. Subsequently they were printed in a collected form in my "Science Series," after having been submitted to the able editorship of Mr. F. L. Pope. In this form I printed 1,000 copies, and after paying the cost of the cuts, paper, printing and binding, when all are sold, at the small retail

price of 50 cents, at which they were placed, there will be but a very inadequate margin of profit on the venture. The author seems to ignore the fact that the magazine in which his lectures appeared in this country, and which magazine is conducted at considerable annual loss to the publisher, first gave publicity to them here, and which the subsequent reprinting of them in the "Science Series" helped to enhance, and to this fact, let me assure the author, is due very much of his reputation, as well as of the demand for his larger work in the United States. D. VAN NOSTRAND.

MISCELLANEOUS.

MECHANICAL USES FOR NATURAL GAS.—At many of the wells near Pittsburgh the natural gas issues with an initial pressure of 200 lbs. to the square inch, or even more, and before it can be used as fuel or illuminant, must have this pressure considerably reduced. where the pipe lines are of any great length, the friction of the gas against the sides and angles is sufficient to accomplish the purpose; but where the fuel is used directly from the well, or where the transit is but short, mechanical devices become necessary. It is now proposed, however, to make use of the force thus stored up in the compressed gas, instead of wasting it, as heretofore, or making provision for its dispersion. One plan suggested utilizes the pressure for blowing blast furnaces, thus dispensing with the enormous engines now employed for that purpose. Sufficient air would, of course, have to be introduced along with the gas to furnish the oxygen necessary for its combustion, and for so much of the solid fuel in the furnace charge as was not oxidized in the reduction of the ore or combined in the resulting pig iron. Should this plan prove practicable, it would also lessen to a great extent, the amount of solid fuel in the burden, and would be a preliminary step in the solution of the problem of a gas blast furnace. Another proposition is to make use of the gas in working engines similar to those using compressed air. The gas, after giving up its stored mechanical energy, would be equally available for the production of light or heat, and its entire power would be utilized. If the supply of natural gas proves at all permanent, it promises to become daily more valuable. Mr. Andrew Carnegie, in his description of the Pittsburgh field, mentions one well, in the Murraysville district, which yields 30,000,000 cubic feet of gas in twenty-four hours. Although this is exceptional, there are many which have a daily output of half this amount, and within a radius of from fifteen to twenty miles around Pittsburgh there are four distinct gas-producing districts. The only question to our mind is whether this supply and pressure will be permanent.

TRANSMISSION OF DRAWINGS BY SIGNAL.—Lieutenant A. Glen, of the Inns of Court Volunteers, read a paper on this subject at the Royal United Service Institution on January 15th. The author, in introducing his subject, said he had been induced to take up the question of army signaling at the suggestion of Colonel Bonham, district signal officer, who expressed the opinion that it was a subject that might well be taken up by volunteers with advantage to the regular army. At the Easter review in 1884, Lieutenant Glen, being in charge of the Inns of Court signaling party, put his scheme in practice, and was able to test its merits and discover its defects, most of which had since been remedied. Lieutenant Glen explained that the object of the system is that one person who has the means of communicating with another by telegraph, heliograph, lamp, flag, or other mode of signaling, may enable the recipient of the signals to make a facsimile of any drawing which may be in the hands of the sender. The drawing may be of any kind, from a rough sketch or plan to a photographic likeness or a chromo-lithograph. The accuracy with which the drawing is transmitted may be increased to any extent that the sender may think fit, while the scale on which the facsimile is drawn is at the discretion of the recipient. The system is based upon the representation of the position of a point by its Cartesian co-ordinates approximately. Polar co-ordinates might be used in almost the same manner, but it would then be found that the accuracy with which the position of a point was represented increased as its distance from the pole diminished, and this peculiarity would be advantageous in signaling the diagrams of a rifle match. Having described the working of his system with the aid of a number of diagrams, Lieutenant Glen gave interesting details of several experiments that had been made with a view to prove the accuracy by which a drawing might be signaled. One of these experiments was the sending by message a likeness of Colonel Bulwer, Q. C., M. P., which was reproduced by the recipient on a larger scale, and recognized upon its completion by those present as a good portrait. In the discussion that followed, Colonel Melville, R. E., briefly described a system which he had patented, having the same object as that invented by Lieutenant Glen, and Mr. H. L. Pilkington adverted to the valuable aid that might be rendered to an army in the field by means of such processes. The Chairman (Colonel Moncrieff), in moving a vote of thanks to the lecturer, observed that he had managed to invest the technical details of his subject with considerable interest, and had made many valuable suggestions, which showed with what assiduity he had studied the question of army signaling.

THE USE AND ABUSE OF THE INDICATOR.—At the meeting of the Manchester Association of Engineers, held on January 23, Mr. W. H. Bailey in the chair, an interesting paper on "The Use and Abuse of the Steam-Engine Indicator" was read by Mr. James Hartley. In the course of his paper, Mr. Hartley pointed out that, although the principal use of the indicator was to exhibit the behavior of the steam in the cylinder of an engine, this was by no means the only purpose to which it could be advantageously applied. In fact it afforded the sole means of exhibiting and recording the changes of pressure that took place in any chamber in which an elastic fluid was confined. It was well known that the action of steam-en-

gines working expansively, especially with small pipes and valves, produced pulsations in the boiler sometimes of a dangerous character, and the extent of these pulsations could only be shown by the application of the indicator. The indicator was also useful to apply on the delivery pipes of certain classes of pumps, to ascertain whether or not the pumps were working satisfactorily, which, in a great many cases was questionable. If engineers and engine tenters would only devote a little more time to the study of the use of the indicator, and the diagrams taken by it, there would be very few of the wasteful engines that were often referred to, and we should be able to obtain the maximum amount of power for the minimum amount of fuel consumed. He wished, however, to point out emphatically that reliable diagrams could not be obtained unless the following conditions were complied with: 1. A good indicator in thorough working order. 2. A careful and competent operator. 3. Suitable tap placed at each end of the cylinder, not loop pipes, with a three-way tap in the center, as, however convenient these may be, they were not satisfactory, as the diagrams must certainly be somewhat inaccurate on account of leakage at times, also from the fact that the steam had a longer distance to travel before acting on the piston of the indicator, especially in engines with long strokes. 4. A good, sharp, metallic point, and good paper; and, lastly, a correct method of giving motion to the barrel of the indicator. If all these conditions were not complied with, unsatisfactory and unreliable diagrams would be the result. The president said they had had a very practical and useful paper. Progress in engineering was very much dependent upon the delicacy and accuracy of the instruments they had to use, and it was in the knowledge of differences that scientific men showed their ability. Mr. Lavington Fletcher said the indicator was a most useful instrument, but might be abused, and the loop pipe was a great trap, and very deceptive. The indicator would not always tell them the amount of steam passing through the engine. Very often, when a complete test was made, a discrepancy of 30 per cent. was discovered; and if any member of that association could invent some kind of meter that they could apply to the hot overflow of an engine, it would be invaluable. They must not be content with the indicator as it is, and he hoped some one would try the testing of the heat that passed out of the engine. Mr. Lewis said, to his mind, the indicator was by no means perfect, and no system of levers could be reliable. He thought some instrument might be devised for communicating the motion of the engine direct to the indicator. Mr. Taylor said that, for very high speed engines, it was a question whether anything in the present shape of indicators was altogether reliable. The primary use of the indicator was that it should record the exact pressure of the steam at any particular part of the cylinder. The usual vote of thanks closed the proceedings.

PROFESSOR CORFIELD, the medical officer of health of St. George's, Hanover square, has presented to the governing body of the district—which includes Belgravia, Mayfair, and the central parish of St. George's—an interesting account of matters relating to health in that part of London. In the completed twelve months under notice, while the death rate of London as a whole stood at 20.3, and of twenty-eight large towns in England at 21.6 per 1,000 of population the rate of all St. George's stood at the low figure of 16.30. The low death rate of St. George's is not only in strong contrast with the rates in other parts of London, but also with some of the large English, Scotch and Irish towns. Preston, in Lancashire, has the largest death rate of the English towns, standing at 27.3 per 1,000 of the population, and is closely run by Manchester, with 26.4, which is again closely run by Liverpool, with 25.2. Glasgow had a death rate in 1884 of 26.9, and Dublin 27.5. There were only five towns in England which had a lower death rate than St. George's.

THE Minot's Ledge lighthouse near Boston, U. S., harbor, is a solid granite structure 200 ft. high, and in a recent gale was severely tried. The keeper of the lighthouse says: "The gale increased constantly, and on Wednesday night we could not sleep on account of the noise. Everything placed against the walls rattled and the thunder of the sea was terrific. Thursday morning I was in the watch-room, just below the lantern, when a sea struck, breaking heavily against the solid granite wall, and dashed its spray and foam 40 ft. to 50 ft. above the pinnacle. The spray from nearly every wave broke over the tower, but none seemed to have a force equal to this. We thought it the heaviest gale at that time the lighthouse had ever experienced, but still the winds went on increasing and the shocks were of greater power. At 2.30 o'clock, and just about high tide, another tremendous wave struck it, still heavier than the one in the night, starting the paint from between the cracks in the ceiling of the watch-room and moving about in all directions. This was the last great effort to beat down the structure, and soon after the gale began to abate."

THE TALLEST CHIMNEY IN THE WORLD.—In the summer of 1884 a new chimney stack was commenced by the Mechernich Lead-Mining Company, which had reached a height of 23 meters when the works were discontinued on account of the autumn storms commencing. On the 14th of April, 1885, the building was resumed, and on the 19th of September following, the total height of 134.6 meters was reached. The leading dimensions are as follows: The foundation, in dressed stone masonry, is 11 meters square, and 3.5 meters high. The base, a cube of 10 meters, and the octagonal plinth of the shaft, are built of annular-kiln bricks. The shaft, of circular form, in radial bricks, is 121.1 meters high, 7.5 meters outside, and 3.5 meters inside diameter at the base, and 3.5 meters outside and 3 meters inside diameter at the top. The height, of 134.6 meters (441.6 ft.), is 2.1 meters (6.9 ft.) more than that of Tennant's chimney, at St. Rollox, which is given as 132.5 meters (434.7 ft.).